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THE STIMULUS-ERROR*

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The purpose of this paper is to discuss the "stimulus-error," to indicate something of its history (though limits of space will preclude more than a bare outline), to add something by way of definition (since definition has remained implicit and there are some who do not understand this term), to enquire, at the level of the scientific experiment, into the significance of the attitude which is thus styled an "error" (relying as much upon experimental observation and as little upon epistemological conviction as is possible), and to arrive, if may be, at an evaluation of the stimulus-error or stimulus-attitude in its relation to the psychology of the present day. This is not so large an order that it does not need filling. Some psychologists put out of court experiments that involve the stimulus-error; others refuse to see any 'error' at all and discount the works that stress this 'merely epistemological' distinction. And when we seek a sanction for the one view or the other, we are at a loss whither to turn, for the "stimulus-error," although it has a long history, has been left to make its way without any very formal introduction.

* The present paper is the outcome of a promise to deal specifically with the nature of the stimulus-error, especially with its relation to psychological measurement and psychophysics. *Cf.* the discussions of the present writer in The logic of the normal law of error in mental measurement, *Amer. J. Psychol.*, 1920, 31, 1-33, esp. 27ff.; and in The control of attitude in psychophysical experiments, *Psychol. Review*, 1920, 27, 440-452, esp. 447f., 449 and note.

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The Stimulus-Error

Undoubtedly much of the confusion and disagreement has been brought about by the term itself: "stimulus-error." It implies something that is right and something that is wrong, defending one position and impugning another. It serves, and was intended, to throw two positions into contrast, to insist upon an important distinction that is often overlooked; yet does not stop with definition, but goes on to pass a judgment. In this dual function of the phrase there has been both an advantage and a disadvantage. To those who accept both the implied distinction and the explicit evaluation the notion has been exceedingly useful, for it has enabled them, not only to separate the methodological sheep from the goats, but also to dispense with the goats,—a telescoping of procedure that is convenient and economical. On the other hand, those psychologists who have staked their fortunes on the goats of stimulus are not to be reformed by being found in the way of the "stimulus-error." They simply deny the "error" and in so doing miss the more fundamental distinction between opposing positions that must be made out before judgment can be passed upon either. We ourselves must not be thus misled, whatever our ultimate judgment may be.

This implied opposition, which we must now bear clearly in mind, is the fundamental opposition in psychology—or between psychologies—of mental process and meaning, of content and object, of *Beschreibung* and *Kundgabe*. Titchener, who is responsible for the term "stimulus-error," puts the case thus:

> "We are constantly confusing sensations with their stimuli, with their objects, with their meanings. Or rather—since the sensation of psychology has no object or meaning—we are constantly confusing logical abstraction with psychological analysis; we abstract a certain aspect of an object or meaning, and then treat this aspect as if it were a simple mental process, an element in the mental representation of the object or meaning. . . . We do not say, in ordinary conversation, that this visual sensation is lighter than that, but that this pair of gloves or this kind of grey paper is lighter than this other. We do not say that this complex of cutaneous or organic sensations is more intensive than that, but that this box or package is heavier than this other. We do not even say, as a rule, that this tonal quality is lower than that, but rather that this instrument is flat and must be tuned up to this other. Always in what we say there is a reference to the objects, to the meaning of the conscious complex. It is not the grey, pressure, tone, that we are thinking of; but the grey of leather or paper, the pressure of the box, the pitch of the violin. . . . What is more natural than to read the character of the stimuli, of the objects, into the 'sensations' with which certain aspects of the stimu-

lus or object are correlated? . . . This is what Fechner did. . . . [He] transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology."[1]

We commit the stimulus-error if we base our psychological reports upon objects rather than upon the mental material itself, or if, in the psycho-physical experiment, we make judgments of the stimulus and not judgments of sensation. At the more complex levels we may make a similar error, a 'meaning-error,' which consists of describing objects, reporting meanings, stating *Kundgabe,* instead of describing mental process or giving *Beschreibung.* We can not, however, in this paper, extend the discussion to include this complex level, but must content ourselves with the conviction that whatever applies in the controversy between judgment of stimulus and judgment of sensation, applies also to introspection and its rival, the statement of meaning. We may concede that the psychophysical experiment in its simplicity represents the ideal ultimate in the psychological experiment, where control of conditions and adequacy of observation are maximal; and that we should be glad to reduce all psychological observation to this degree of rigor at least. At any rate any extension of this discussion to the 'higher' processes must wait, for the history and application of the stimulus-error are at the level of psychophysics, and the interpretation of the "stimulus-error," up to which we are leading, shows most clearly here.

The Quantity Objection and the Stimulus-Error

It is not surprising that a psychophysics, which seeks to establish the relation between the mental and the physical, should emphasize the distinction between sensation and stimulus. What is surprising is that the opponents of psychophysics should have raised this very distinction for the confounding of psychophysics and should have claimed that the psychophysical relationship (the logarithmic relation of the Weber-Fechner law) was an artifact created, not by the attempt of the psychophysicist to distinguish between sensation and

[1] E. B. Titchener, *Experimental Psychology,* II, i, 1905, p. xxvif.; *cf. Text-book of Psychology,* 1910, 202f. Titchener first uses the term stimulus-error = "R-error" in *Exper. Psychol.,* II, ii, 1905; see pp. lxiii, 198f., 203ff., 207, 219, 223, 230f., 262, 450. For Titchener's further use of the term, see *Text-book,* pp. 218, 350, 398 note, 522. J. v. Kries characterized the objectifying attitude as a "source of error:" "Wenn man aber diese Quelle des Irrthums ausschliesst und möglichst an den objectiven, als Reiz dienenden, Vorgang gar nicht denkt . . . ;" *Vtjschr. f. wiss. Philos.,* 1882, 6, 275; and Titchener seems to refer to this discussion as a sanction for the term "stimulus-error."

stimulus, but by his confusion of the two. Yet such is the substance of the "quantity objection" to psychophysics, which had later to be met with the psychological sense-distance by Müller, Titchener, and others, who thus turned the tables and brought the argument for the distinction between mental and physical material to the support of a Fechnerian psychophysics. This was a long and tedious battle, and one might have expected that the resultant emphasis upon the two-fold nature of psychophysics would have determined the psychophysical universe for a time. On the contrary, however, the confusion between sensation and stimulus persisted. Cattell was fathering a psychology of the stimulus, and it was in the tradition of the work of Fullerton and Cattell that Urban did his experiments. Now that behaviorism has come into vogue, it is not apparent that we do not have two kinds of psychophysics—a psychophysics of process that gives, as Fechner wanted, the correlation between mental and physical data, and a psychophysics of behavior that seeks to identify response with its stimulus. That this psychophysics of stimulus-and-response needs also, if it is to be scientific, to take account of the error that has been called the "stimulus-error" is the thesis of the present paper; but the thesis must wait upon the perspective of the preface.

A clear recognition of the distinction between mind and body, between consciousness and objects, was the key-note of Fechner's position. There was for him at least this dualism in the universe, which may be regarded from one standpoint or the other. The case is not unlike, Fechner argued, the Ptolemaic and Copernican worlds. The geocentric and heliocentric solar systems are different systems, and we may at pleasure take either point of view that we choose. The worlds remain distinct. Or the matter is like a circle, which may be viewed from the inside or the outside. In the one case we see only concavity, in the other only convexity. Such a dualism can be resolved only by the law of relationship that holds between its two aspects, and, just as the relationship between concavity and convexity can be stated geometrically for the circle, so the logarithmic law resolves the dualism of mind and body. There is no doubt, therefore, that Fechnerian psychophysics stands or falls according to its success in distinguishing between measurements of mind and measurements of body, or between sensation and the object of sensation, the stimulus.[2]

[2] G. Th. Fechner, *Elemente der Psychophysik*, esp. 1889, I, 1-12.

The vigorous opposition that developed to Fechner's psychophysics took its stand firmly upon the distinction between mind and body, but denied the possibility of a quantitative correlation between the two on the ground that mind was not possessed of magnitude and that mental measurement was an impossibility. This argument came to be known as the "quantity objection" and was the main source of opposition to quantitative psychology in the eighties and nineties of last century. Introspection, the objection runs, does not show that a sensation of great magnitude ever contains other sensations of lesser magnitude in the way that a heavy weight may [supposedly] be made up of a number of smaller weights. "Our feeling of pink," said James, "is surely not a portion of our feeling of scarlet; nor does the light of an electric arc seem to contain that of a tallow-candle in itself."[3] "This sensation of 'gray,'" remarked Külpe, "is not two or three of that other sensation of 'gray.'"[4] "A blue surface," Ebbinghaus commented, "is something other than a green, but the latter has in itself, apart from memory of the colors, nothing of the doubleness or threefoldness of the green. . . . A low tone sounds different from a high tone, and in like manner a loud tone different from a soft."[5] In other words increase of magnitude in no sense means increase of complexity. A sensation is just itself no matter what its degree. The tone produced by many instruments in unison is not of itself composed of more units than is the tone from a single string, nor is the tone of many vibrations per second more complex than the tone of few vibrations. In this form the objection seems obvious enough. Sensational magnitude is certainly not multitude, and intense sensations are not integrated of more sensory stuff than are weak.[6] How then was psychophysics to defend itself?

Its immediate defense was a display of the factual material. Here were the experimental measurements. If they were not observations of the magnitudes of sensation, what were they?

[3] W. James, *Principles of Psychology*, 1890, I, 546.

[4] O. Külpe, *Outlines of Psychology*, tr. 1896, 45.

[5] H. Ebbinghaus, *Z. f. Psychol.*, 1890, 1, 323.

[6] Titchener mentions as raising the quantity objection: G. E. Müller in 1878, Exner in 1879, Stadler in 1880, Zeller in 1881, Boas and F. A. Müller in 1882, Stumpf in 1883, Tannery in 1884, Elsas in 1886, Grotenfelt in 1888, James, Münsterberg, and Ebbinghaus in 1890, Sully in 1892, Külpe in 1893, Wahle in 1894, Meinong in 1896, Höfler in 1897, and Lehman in 1902. See Titchener, *Exper. Psychol.*, II, ii, pp. xlviii-lxiii.

To this question the raisers of the "quantity objection" replied that psychophysicists had created an artificial mental magnitude by a confusion of the sensation with the stimulus, that is to say, they had committed in their experimental work the "stimulus-error." This was a serious charge against a discipline that depended for its existence upon a sharp distinction between the mental and the physical. Let us see how the accusers dared to raise it.

Von Kries put the matter clearly:

"An illusion is thus very easily brought about by the fact that one tends in general to estimate objective values (measurable in objective terms) according to the sensation. If one, however, excludes this source of error, and in so far as possible thinks not at all of the objective process serving as a stimulus, then one must necessarily admit that a quantitative relation does not exist between the different parts of an intensive series. This fact is most obvious to us when we do not attempt objectification as, *e.g.*, in pain. Whatever it is called, a pain exactly ten times as strong as another does not admit of such absolute statement."[7]

Ebbinghaus, somewhat later, was even more explicit:

"In general one designates the brightness of a flame or a surface as 10 or 12 times another brightness, and could just as easily, it appears, designate a loud tone as the double or treble of a soft tone. But what occurs here is no longer an immediate sensation or an immediate judgment of sensations, but depends upon the introduction of experiences. We can readily experience, and we do every day experience, the fact that the arousal of a brightness or a loudness depends upon a diversity of just those physical things or processes that in limited number call forth the impression of darker or softer. In order to have an impression of greater brightness for a surface, one can increase the number of gas-flames illuminating it; in order to strengthen a tone, one multiplies the instruments carrying it. Such experiences with respect to the causes of sensations we have always in immediate view, and we believe that we have the numerical characteristics that always attach to the one occurring without anything further in the other. It is psychologically difficult to get rid of them, just as it is difficult not immediately to see in a grass-green apple its sourness. But if one succeeds in the perfectly possible separation of the thought context, then it is clear that, as the bare visual impression of an apple has no sourness in it, similarly the bare impression of brightness does not consist of the multiplicity of candles upon which, of course, it frequently depends."[8]

We have already seen what fifteen years later, Titchener had to say in the same vein and how, although defending mental measurement, he makes the charge of the stimulus-error against Fechner. And there were many others.

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[7] J. v. Kries, *loc. cit.*

[8] H. Ebbinghaus, *op cit.*, 323f.; *cf. Grundzüge der Psychologie*, I, 1905, 71-79.

Exner put forward the general argument in 1879;[9] and Boas in 1882.[10] Tannery said in 1883: "It is the objective study of the excitation and its variations that leads to this definition of number that measures the sensation. At bottom it is by excitation that sensation is defined."[11] On epistemological grounds both F. A. Müller[12] and Meinong[13] concluded that mental magnitudes, unlike physical, were indivisible. And long before any of these, Brentano, the father of modern intentional psychology, had said: "If one measures, as Fechner did, the intensities of colors, tones, *etc.*, then one is measuring the intensities of physical phenomena. The color is not the seeing, the tone is not the hearing, the warmth is not the sensing of warmth."[14]

Nevertheless this still seems a surprising charge to bring against Fechnerian psychophysics. If the fundamental task of psychophysics is the discovery of the relationship between the hitherto unrelated body and mind, is it not astonishing that psychophysics should have confused the two, the two whose very separateness was the *raison d'être* of psychophysics? Yet the critics stuck to the point and were at pains to show the readiness with which these incommensurables did duty, the one for the other. Ward, pointing out that the psychophysical limen expressed in terms of stimulus was physical quantity, concluded: "There is no trespass harder to avoid than that across the lines dividing the subjective and objective aspects, and none more disastrous to the offender."[15] Other writers urged the same point, and Külpe even brought the prevalence of objectification into an experimental study.[16] It is no wonder then that objectification was thought of as a source of error and that Titchener coined for it the term "stimulus-error."

The Answer to the Quantity Objection

The fundamental and final answer to the quantity objection was Weber's law: S=k log R. In so far as the relationship had been observed, no amount of explaining could explain it entirely away. It might be that the function was not exactly

[9] S. Exner, Hermann's *Handbuch der Physiologie*, 1879, II, ii, 242.
[10] F. Boas, *Pflüger's Arch.*, 1882, 28, 568f.
[11] J. Tannery in J. Delboeuf, *Eléments de psychophysique*, 1883, 138.
[12] F. A. Müller, *Das Axiom der Psychophysik*, 1882, 46-56.
[13] A. Meinong, *Z. f. Psychol.*, 1896, 11, 81-133, esp. 96ff.
[14] F. Brentano, *Psychologie vom empirischen Standpunkte*, 1874, I, 91.
[15] J. Ward, Mind, 1876, O. S. 1, 460.
[16] O. Külpe, Ueber die Objectivirung und Subjectivirung von Sinneseindrücken, *Philos. Stud.*, 1902, 19, 508-556.

logarithmic or that it held only within certain limits; it might not be certain just what was the nature of S, or of R; but the unescapable fact was that there were an S and an R, which were covariant, and which were not identical since the mode of variation of the one was not the mode of variation of the other. To charge the stimulus-error and say that S was contaminated by R was not enough, since the confusion of S with R was not enough to explain the discovery of this difference in variation. The psychophysicists, therefore, had the stronger position, and had only to show where the difference actually lay. There seem to be five ways of accounting for the difference and thus of establishing psychophysics.

1. Systematically one may argue for a physiological interpretation of Weber's law, as Müller did.[17] Excitation varies somewhat as does the logarithm of the stimulus. No one doubts that excitation may have magnitude, and thus the quantity objection is met. Moreover the logarithmic relation between physical dependents is not unknown.[18] Excitation, however, does not happen to be open to immediate observation, so we must observe its correlate sensation. We deal therefore with stimulus and sensation, which we must keep apart, avoiding the stimulus-error; and we escape from the formal objection that sensation does not have magnitude by making it a mere qualitative indicator of excitation which must have magnitude.

2. Wundt's psychological interpretation of Weber's law meets the quantity objection by the introduction of *Merklichkeitsgrade*. Sensations do not have magnitude, but if they did the matter would be irrelevant to psychophysics. It is apperception that gives a quantitative aspect to mind; there are degrees of noticeableness to sensations or to the differences between them.[19] The sensation scarlet is not more than the sensation pink, but is more noticeable than the pink; and the difference between a scarlet and a pale pink is more noticeable than the difference between the scarlet and a rose. To introspection it is just as obvious that apperception has degree as it is obvious that sensation has not, and it is between these

[17] G. E. Müller, *Zur Grundlegung der Psychophysik*, 1878, 224-403.

[18] Müller, *loc. cit.*; Ward, *op. cit.*, 452-466; Titchener, *op. cit.*, II, ii, 66f. The autocatalytic theory is more recent: T. B. Robertson, *Monist*, 1909, 19, 372ff., 384f.

[19] Wundt's theory passed through successive stages and no brief statement does it justice. For summary and discussion, see Titchener, *Exper. Psychol.*, II, ii, pp. lviff., lxxivf., lxxxff., 69f.; for summary and genesis, p. lxxxii. note.

Merklichkeitsgrade and the physical values of the stimulus that the logarithmic law holds.

3. What was Fechner's answer to the quantity objection? To deny the stimulus-error, which the quantity objection implies. In the *Revision* he wrote:

> "One must take care not to try to count relations that exist in the physical realm between physical units as existing within the mental province because they can be grasped only by the mind; for in so doing one loses the ground for distinction between the two provinces. Even the physical, within which the relations exist, must be grasped by our minds in order to exist for us and to be spoken about. Yet we discriminate on the basis of this community between outer and inner phenomena as between two provinces, and have to distinguish properties as belonging in the one or the other, not merged with each other or interchangeable, as might sometimes seem to be the case under a philosophical point of view. At any rate psychophysics takes this point of view and thus avoids confusion. Whenever something like a relation, a change, a difference, a unity, or a fusion, *etc.*, appears as characterizing the physical or psychical world, it is abstracted from the province of the one or the other, or it is counted into the one or the other province; it may occur just as readily in psychology as in natural science. Hence it is perfectly possible that the pitches should be represented in one province by something in the other province without our identifying the two. The relations of periodicity between vibrations, which occur as the psychophysical representatives of melodic and harmonic sensations, are the most obvious; the one is something very different from the other."[20]

In other words it is all in our point of view. We can judge the stimuli or we can judge the sensations; and, according as we do the one or the other, we constitute for ourselves the physical or the mental world. It is nonsense to assume that, because we make judgments of physical phenomena in building up natural science, this natural science is a science of judgments and therefore mental. The two are distinct, and the discovery of the difference that is summarized by Weber's law attests the distinction.

To the writer of this paper it seems that Fechner's argument, turned a different way, becomes at least as invincible as any of the other ways out of the difficulty. The trick for escaping the force of the quantity objection, when directed against mental phenomena, is to turn it upon the stimuli themselves. Suppose sensations of weight do not under observation exhibit magnitude; what of the physical weights themselves? To physical observation ten grams is *a* weight and one gram is *a* weight; it is only in common sense, which is assuredly not physics, that ten grams is ten one-gram weights. Because physical phenomena, like mental, are referable to

[20] Fechner, *Revision der Hauptpunkte der Psychophysik*, 1882, 5f.

objects, is no excuse for reading the objects into them. Physical weight is as little the number of objects in the scale-pan as mental weight is the number of weights in the hand. The physical quantity is just as simple and unitary as the mental, and if sensation lacks magnitude so must stimulus. We can hardly, however, deny measurement to physics, and it thus appears that the quantity objection is not valid either against the measurement of sensation.[21]

4. Undoubtedly the most general way of meeting the quantity objection while saving mental measurement is by the substitution of the sense-distance for the sensation magnitude. Historically this conception dates from Delboeuf's *contraste sensible*. It is not necessarily incompatible with any of the foregoing accounts of mental measurement and is endorsed essentially by Wundt, Boas, Stumpf, Ebbinghaus, James, Meinong, Höfler, Stout, and G. E. Müller.[22] It is the basis of Titchener's quantitative psychology, where it finds its clearest exposition.[23]

This position holds that sensations, although they do not possess magnitude, may lie within a continuum, and that, although we can form no quantitative estimate of any sensation, we can nevertheless estimate the relative degree of separateness of two sensations within the continuum. Sensations are simply themselves and are not summed of various numbers of increments; the distances between these sensations, however, do vary and can be estimated in amount. The simplest case of mental measurement occurs when, for a series of three sensations, A, B, and C, occurring in a continuum, we estimate the sense-distance AB as equal to the sense-distance BC. Here we have measurement, for we have laid off the unit AB=BC twice in the distance AC, and it is the correlation of such estimated sense-distances with the corresponding values of stimulus that gives Weber's law.

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[21] The more thorough exposition would show that magnitude and measurement are systematic matters and are not found immediately at the observational level of science. The confusion is not unlike that of the systematic 'sensation' with the observational 'attribute:' *cf.* Sensation and system, *Am. J. Psychol.*, 1915, 26, 258-267, where Titchener makes this point. On the other hand, it is hardly fair to physics to say: "No sensation is a sum of sensation-parts or of sense-increments; no sensation is a measurable magnitude. Fechner has transferred to sensation a point of view that is right for stimulus, but that introspection refuses to recognize in psychology:" Titchener, *Exper. Psychol.*, II, i, p. xxvii.

[22] *Cf.* Titchener, *op. cit.*, II, ii, p. cxxxiii.

[23] Titchener, *op. cit.*, II, i, pp. xxi-xxvii; ii, pp. cxvi-cxliv.

5. All the foregoing modes of meeting the quantity objection are successful without sacrificing the possibility of mental measurement; the fifth mode of defense consists in joining the enemy. We can give up the measurement of mind, substituting the measurement of sensitivity or of capacity-for-discrimination. Fullerton and Cattell give us our orientation here.

They declare, in the first place, that both sensation magnitudes and sense-distances are undiscoverable:

> "If an observer can, in fact, estimate quantitative amounts of difference in sensation, apart from association with known quantitative differences in the stimuli, a relation between mental and physical intensity can be determined. The writers, however, agree in finding that they cannot estimate such quantitative differences in sensation in a satisfactory manner. We can indeed say when one weight seems approximately double another, but this is doubtless because we have often lifted first one volume, and then two, and the like. But we cannot say when one sound seems twice as loud, or one day twice as hot as another. We have made experiments to see how nearly different observers would agree in adjusting one shade of light midway between two others, and have found hesitation in coming to a decision and great divergence of opinion. Most men will think that a just king is happier than a tyrant, but few will agree with Plato in considering him 729 times as happy."[24]

What is left? The observed *stimuli*[25] and errors of observation incurred in observing the stimuli.[26] There is no constant just noticeable difference nor threshold.[27] We have only errors of observation as we fail to observe an actual difference in the stimuli or, less often, observe a difference that is not there. These errors can be treated under the ordinary calculus of probabilities and follow the normal law of error.[28] The amount of the average error is always determinable and it increases with the magnitude of the stimulus.[29] It is the law of the dependence of the average error of observation upon the magnitude of the stimulus that Weber's law seeks to state, although the mathematical form of Weber's law is actually incorrect.[30]

Such a quantitative psychology of error is of necessity a psychology of capacity,[31]—of the capacity of the organism to

[24] G. S. Fullerton and J. McK. Cattell, *On the Perception of Small Differences*, 1892, 20.
[25] Fullerton and Cattell, *op. cit.*, 9ff., 20, 153, *etc.*
[26] Fullerton and Cattell, 14ff.; Cattell, *Am. J. Psychol.*, 1893, 5, 287ff.
[27] F and C, 11, 150; Cattell, 288f.
[28] F and C, 12ff.; Cattell, 285ff.
[29] F and C, 23ff., 153f.; Cattell, 290ff.
[30] F and C, 24ff., 152.
[31] *Cf.* Titchener, *Exper. Psychol.*, II, ii, p. cxxxiv note.

respond correctly to stimuli. This point of view has since developed into the statistics of mental tests and of behaviorism, which is a psychology of stimulus and response. It is a point of view for which there is no stimulus-error since quantitative judgments can be made only of the stimulus, and it is one that touches other sciences very closely since it concerns itself with observation, the method of every science.[32]

The Two Psychologies

Thus it becomes evident that the answers to the quantity objection have divided along the traditional cleft in psychology. We have not only a psychology of datum and a psychology of capacity, but we have quantitative psychologies of datum and of capacity.[33]

The quantitative psychology of datum ["the given"] insists upon a truly mental measurement. When further it correlates these mental measurements it is the true psychophysics. It may answer the quantity objection in any of the first four of the five foregoing ways, because any one of those four insists upon or explains the existence of mental quanta. It is a part of the larger psychology that is variously called structural or introspective, the psychology of process or of *Beschreibung*.

The quantitative psychology of capacity admits the quantity objection and denies—or at least ignores—mental quanta. This psychology sees no distinctively mental measurement, but undertakes the physical measurement of bodily response as a function of the physical quantities of the stimulus. There is no sharp epistemological line discernible between this sort of measurement and other physical measurement, and it thus meets the requirement of modern behaviorism that psychology interpenetrate physical science without sensible demarcation.[34] The psychology of capacity is also the psychology of mental tests[35] and of Urban's psychophysical experiments.[36] These

[32] F and C, 9; Cattell, 285.

[33] *Cf.* O. Külpe, *Vorlesungen über Psychologie*, 1920, 5ff.; Titchener, *Am. J. Psychol.*, 1921, 32, 108-120.

[34] J. B. Watson, *Psychol. Rev.*, 1913, 20, 177; *J. Philos., etc.*, 1913, 10, 427; *Psychology*, 1920, vii.

[35] See note 31 above.

[36] The present writer has already had occasion to refer to F. M. Urban's position: E. G. Boring, *Am. J. Psychol.*, 1920, 31, 27f., esp. note 77; *Psychol. Rev.*, 1920, 27, 446f. It is well to repeat that no reference is intended here to Urban's later position, which involves an acceptance of the epistemology of Mach and Avenarius; see Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.*, 1913, 30, 113-152, esp. 113, 124f., with notes; *cf.* also *Am. J. Psychol.*, 1913, 24, 274.

latter seem strange mates, but the influence of Cattell is apparent in both. If the germ of the tests was in Galton, nevertheless it was Cattell, in the interests of the psychology of individual differences, who planted it in American soil where it has brought forth fruit abundantly. Urban's origin is less apparent. It is trivial to remark that he began his psychophysics in the laboratory that Cattell had founded years before. The evidence is internal and not explicit, but the seeker after information can find it.[87] In the broad, we may add, the psychology of capacity may become a functional psychology and deal, when it is experimental, with *meanings* and *Kundgaben* instead of mental processes and *Beschreibung*.

As a term the "stimulus-error" is the property of the psychology of datum. It characterizes the attitude of the psychology of capacity as being concerned observationally with the stimulus and admonishes against it. Of course the psychology of capacity ignores this admonition, for in making observations of the physical stimulus it is simply fulfilling its self-appointed task. The "stimulus-error" is no valid charge against this psychology, nor does it raise within it a question of right and wrong. No more can be expected of either psychology than that it hold to its premises.

In practice there is a difficulty, however. No matter how distinct the two psychologies may be, psychologists can scarcely hold strictly to the one or to the other. The psychologist of the datum can not be expected to attack a new perception without recourse to judgments of stimulus or of meaning,[88] and statements of meanings moreover may themselves become the object of psychological investigation.[89] As to whether the psychology of capacity might remain wholly faithful to the

[87] On psychophysical judgments and random events, compare Fullerton and Cattell, *op. cit.*, 12ff., 23ff., with Urban, *The Application of Statistical Methods to the Problems of Psychophysics*, 1908, 17f. On the meaning of the just perceptible difference, compare F and C, 11, and Cattell, *op. cit.*, 288f., with Urban, *Stat. Meth.*, 70. On mental measurement compare F and C, 20, 152f., and Cattell, 293, with Urban's ignoring of the issue in *Stat. Meth.* [Or could he have thought that Titchener had settled it for the sense-distance? *Cf. Psychol. Bull.*, 1912, 9, 245.] On judgment as directed upon stimulus, compare F and C, 20, and Cattell, 293, with Urban, *Stat. Meth.*, 5, 17, *Psychol. Rev.*, 1910, 17, 27ff. On recording degrees of assurance, compare F and C, 11, 151, with Urban, *Stat. Meth.*, 5ff. On the relation of psychophysics to physics, compare F and C, 151, Cattell, 285, with Urban, *Psychol. Rev.*, 17, 243f.

[88] *Cf., e.g.*, L. B. Hoisington, On the non-visual perception of the length of lifted rods, *Am. J. Psychol.*, 1920, 31, 114-146.

[89] *Cf., e.g.*, H. P. Weld, Meaning and process as distinguished by the reaction method, *Titchener Commemorative Volume*, 1917, 181-208.

stimulus it is not so easy to say. This attitude is more natural and it is possible to complete entire experiments without once trespassing upon the realm of mental process. On the other hand, the functional psychologies for all they have to say of the inadequacies of structural psychology, seem usually unable to complete the systematic mental picture alone, and a recent system of behaviorism has drawn unhesitatingly upon the psychology that it seeks to supplant.[40] In general what is fundamental to the one can not be ignored by the other, and on this account the writer of this paper would urge the attention of the psychologist of capacity to the "stimulus-error."

The Effect of the Stimulus-Error

If we are now to urge upon the psychology of capacity the avoidance of the stimulus-error, it is a fair demand that we state first the probable penalty that is incurred by a failure to accept our advice. Here we can not stand upon the epistemological ground that psychology observes mental processes and not stimuli, and that judgments of stimulus are therefore *a priori* inadmissible. This historical warning against the stimulus-error does not apply to the psychology of capacity which protests against a scientific dualism and deals by preference with stimulus and response. What we have to show is rather that the stimulus-error works against the establishment of the univocal correlations between stimulus and response that a psychology of capacity demands, that it interferes with the prediction of the response for a given stimulus. Here the ground is broadly scientific: we are dealing with the constancy of experimental conditions and the reproducibility of results.

When we go frankly to the literature, however, asking just what in numerical terms may be the effect in mental measurement of allowing judgments of the stimulus or of instituting them, we meet at first disappointment.

The psychology of datum is set to avoid, rather than to measure, the stimulus-error. It tells us where the stimulus-error is most insidious, *viz.*, in judgments of supraliminal sense distances.[41] We may have trouble with the limens; we are almost sure to have it in comparing large sense-distances. Sometimes a special technique is necessary to avoid the error. In Martius' experiment on the apparent size of objects at different distances from the eye, all the stimulus habits for the estimation of the size of objects in everyday life are

[40] Watson, *Psychology, from the Standpoint of a Behaviorist*, 1919.
[41] *Cf., e.g.*, Titchener, *Textbook*, 218.

appealed to, and a special method is required "in removing the initial tendency of the observers to reflect on the actual size of the comparison rod in relation to the standard rod."[42] In Angell's experiment on intensive distances between sounds one would expect concrete reference to the stimulus to enter much less readily; on the contrary, however, the observers tend to judge the height of fall or the angle of fall of the stimulus, and to neglect the mental datum.[43] For this error special remedies are presented. Müller urges that the amount of intensive difference be taken as the *Kohärenzgrad*, the *Leichtigkeit des Kollectivaufgefasstwerdens* of the two sounds.[44] Titchener suggests letting the observer blunder into the stimulus-error and then rescuing him by individualized treatment, his protocols in hand.[45] These experiments are striking cases, but even the psychologists who have no special measures of reform to offer cry out against the evils of judgments based upon secondary criteria, upon associates of the processes judged, or upon surrogate processes.[46] A surrogate, they complain, can even render incommensurables artificially commensurate, as seems to be the case when the intensities of weights and noises are compared in terms of *Spannungsempfindungen*.[47]

Now it is not likely that there could be so much smoke without some fire; yet we are still at a loss to estimate the amount of danger. It would be reasonable for these psychologists of datum to seek to avoid stimulus for no other reason than that they are interested solely in "mind," but it is not to be supposed that the matter would have been taken so seriously had the quantitative results, and Weber's law which is dependent upon them, been unaffected by the kind of judgment given. Indeed this belief came to the surface when Grotenfelt, in defense of Weber's law, accused Merkel of the stimulus-error and inclined to the belief that results

[42] G. Martius, Ueber die scheinbare Grösse der Gegenstände und ihre Beziehung zur Grösse der Netzhautbilder, *Philos. Stud.*, 1889, 5, 601-617, esp. 605f.; *cf.* Titchener, *Exper. Psychol.* II, ii, pp. 262f.

[43] F. Angell, Untersuchungen über die Schätzung von Schallintensitäten nach der Methode der mittleren Abstufungen, *Philos. Stud.*, 1891, 7, 414-468, esp. 438.

[44] G. E. Müller, *Die Gesichtspunkte und die Tatsachen der psychophysischen Methodik*, 1904, 237f.

[45] Titchener, *Exper. Psychol.*, II, ii, p. 198; *cf.* also pp. 203f., 230.

[46] *Cf.* Fechner, *Elemente*, II, 318ff.; H. Neiglick, *Philos. Stud.*, 1888, 4, 41; Angell, *op. cit.*, 438; W. Ament, *ibid.*, 1900, 16, 173; G. E. Müller, *op. cit.*, 241; J. Fröbes, *Z. f. Psychol.*, 1904, 36, 259.

[47] H. Münsterberg, *Beiträge zur experimentellen Psychologie*, III, 1900, 56-122, esp. 98ff.

that followed Merkel's law might possibly indicate that they were based upon judgments of stimulus.[48] The data that we wish, however,—the comparison in quantitative terms of results of judgments of process—are, in the earlier history of psychophysics, lacking.

(The psychologists of capacity, we may note in passing, are not to be asked for this comparison. Cattell and Fullerton denied the possibility of mental measurement. They can not therefore be asked for its comparison with any form of physical measurement.)

On the basis of recent literature, however, there is something to be said, in answering this question, for the case of lifted weights, and very much to be said for the case of the limen of dual cutaneous impression. We may mention the lifted weights at once and reserve the two-point limen for the next section.

Friedländer undertook a comparative study of lifting weights under different *Einstellungen*.[49] He employed a "*G-Einstellung*" in which the attention was directed upon the lifted object (*Gegenstand*, hence "*G*"), and an "*A-Einstellung*" in which the object was abstracted from (hence "*A*") and the attention directed upon the sensory aspect of the experience (*Druck-, Spannungs-, Kraftempfindungen*).[50] Here we should expect to find the results we are seeking, for the *G-Einstellung* is the stimulus-attitude, the attitude demanded by a psychology of capacity and called the "stimulus-error" by the psychology of datum; and the *A-Einstellung* in its various forms is the process-attitude which avoids the "stimulus-error." There is not the least doubt that the two attitudes give different results. "Differential sensitivity on the whole is somewhat finer for the G-series" for a standard weight of 500 g., Friedländer tells us, but a standard of 1200 g. may give a finer discrimination under the *A-Einstellung*. The data unfortunately are for one observer only. They are based on too few cases,—41 series after the practice-effect was presumably constant. The observer did not always succeed in maintaining the required attitude, for the stimulus-attitude was difficult for him (!) and he sought to make his finer discriminations under the *A-Einstellung*. The resultant psychometric functions are not smooth ogives; one just barely misses inversion in its central

[48] A. Grotenfelt, *Das Webersche Gesetz und die psychische Relativität*, 1888, 111f.; *cf.* Titchener, *Exper. Psychol.*, II, ii, pp. lxxviiif., 219.

[49] H. Friedländer, Die Wahrnehmung der Schwere, *Z. f. Psychol.*, 1920, 83, 129-210, esp. 187-193.

[50] Pp. 133ff.

portion.[61] We are not yet ready, then, to generalize as to the exact effect of attending to the stimulus in making psychophysical judgments. All we can say is that there is an effect, that a shift in the observational attitude alters the numerical results significantly. We shall not dare, therefore, if we wish to predict response from stimulus, to leave attitude out of account.

My colleague, Professor Fernberger, has recently completed similar experiments in the Clark Laboratory. He had three observers, and extended his series through many fractions to take account of progressive practice and to give an adequate number of cases. His resultant ogives are smooth and regular, and his procedure seemed calculated to yield all that could be desired methodically. He gets differences for the different attitudes,—more striking differences in some cases than Friedländer's. More than this I can not say in advance of the publication of his results. Perhaps in his final analysis he will discover a generalization, which is not apparent to casual inspection of the functions and constants. The results indicate unquestionably that an alteration of attitude by instruction may result in an alteration of the psychometric functions, which is significant in the mathematical sense of being many times its probable error, but which for a given observer is unfortunately quite unpredictable. Attitude may be very important even when we can not say just why.

Fortunately the case of the two-point limen is less mysterious.

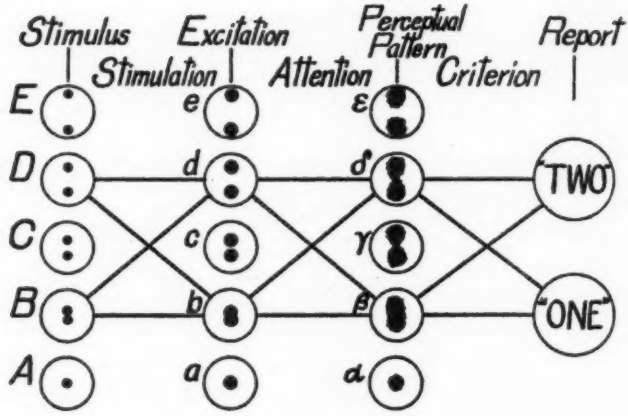
The Stimulus-Error as Equivocal Correlation

The limen of dual impression upon the skin furnishes the case for which we are looking. We know, not only that judgments of stimulus may here make a difference in the quantitative results, but we know further how great this difference may be and something of its conditions. We are in a position, moreover, to generalize from these facts with some assurance and to assert that the effect of the "stimulus-error,"

[61] In fact it is not even clear that Friedländer's cautious generalization is not in part an artifact. If we compute the data of Table 10 by Urban's procedure for the *Konstanzmethode*, we get:

	A-Einstellung	G-Einstellung
Av. measure of precision (h)	0.0138	0.0147
Interval of uncertainty (grams)	35.39	37.13
Point of subjective equality (grams)	500.8	509.9

There is not much difference in precision or discrimination by this method. The striking difference is in the effect of attitude on the point of subjective equality.



from the point of view of a psychology of capacity, is—under similar conditions, at least—to render the correlations between stimulus and response equivocal and thus to jeopardize the rigor of conclusion that science demands.

We may proceed to the point by reference to the visual schema of the figure reproduced herewith. The diagram is intended merely to assist in the analysis of the factors involved and not as an actual picture of neural or psychophysical fact.

One deals in determining the two-point limen with a series of stimuli, *A, B, C, D, E,* pairs of stimulus-points at different separations, with perhaps a single point, *A,* at the extreme of the series. From the work of numerous investigators[52] we know that there is a similar series of perceptual patterns, $\alpha, \beta, \gamma, \delta, \epsilon$, which passes, with approximate regularity, from a sharp point to a blunt point, to an oval, to an elongated oval, to a double-paddle, to a dumb-bell, to two separated points. We may best think of these perceptual patterns as the process material of the psychology of datum, but they exist also for the psychology of capacity as inscrutable "middle terms." Intermediate between stimulus and process we are accustomed to assume some sort of excitatory process, *a, b, c, d, e.* Of

[52] G. A. Tawney, *Psychol. Rev.,* 1895, 2, 585-593; V. Henri, *Ueber die Raumwahrnehmungen des Tastsinnes,* 1898, 6; M. Foucalt, *L'illusion paradoxale et le seuil de Weber,* 1910, 122-145; E. J. Gates, *Am. J. Psychol.,* 1915, 26, 152-157; and numerous other references cited in these articles.

these there must also be a series, and knowing little about them, we may picture them as somewhat like the perceptual processes. Finally there is the verbal report which is generally limited experimentally to the words *One* and *Two*. It seems, moreover, that we keep within the limits of scientific good sense if we say that stimulus, excitation, mental process, and report form a dependent series, and that each is the resultant of the preceding.

If now we choose for special consideration the stimuli *B* and *D*, we may note that they most frequently, perhaps, give rise to the dependent series '*B-b-β-One*' and '*D-d-δ-Two*,' but we must note further that cross-connections are possible.

Stimulus gives rise to excitation by way of stimulation. Here variation may tend either in the direction of fusion or of separation. On the forearm, for example, the angular shifting of stimulus toward the longitudinal axis may result in fusion and *D* may give rise to *b;* if the rotation is toward the transverse axis, *d* may follow upon *B*.[53] Location as well as angular orientation also yields these differences. Near the elbow *D* gives *b*, say; a little further down *B* may give *d*.[54] It is also possible that the chance impingement of the stimulus-points upon multiply innervated spots in the skin gives rise to multiple excitation (*B* to *d*) and that the stimulation of less complexly innervated spots may lead to a simpler excitation (*D* to *b*).[55]

At the next phase of our series excitation culminates in perceptual pattern under the selective action of attention. These terms sound strange of course to the psychologist of capacity, but we may ask him to accept them or to find substitutes for them. They stand for factors that affect the correlations with which he is working, and he must take scientific account of them, whatever he calls them.

It is experimentally demonstrable that under inattention potential *Twos* become *Ones;* one stimulating point or the other catches the attention and thus withdraws it, as it were, from the other.[56] The converse occurs in the well-known *Vexirfehler*, the paradoxical judgment, where two points close together or even a single point give a perception of duality. Henri and Tawney referred this phenomenon in part to attention, and its dependence upon certain suggestive factors

[53] *Cf.* Tawney, *Philos. Stud.*, 1897, 13, 170; *cf.* Boring, *Quart. J. Exp. Physiol.*, 1916, 10, 23.

[54] *Cf.* Henri, *op. cit.*, 26f.; *cf.* Boring, *loc. cit.*

[55] Henri, *op. cit.*, 64ff.; Boring, *op. cit.*, 89-93, esp. 93.

[56] *Cf.* A. Brückner, *Z. f. Psychol.*, 1901, 26, 54f., 60; Boring, *op. cit.*, 88f.

seems to indicate that it is of this order.[57] Griesbach and others supposed that fatigue reduced the limen,[58] but Friedline has shown that the effect of fatigue is operative only among the perceptual forms at the lower end of the series, say α and β. Here fatigue acts in the direction of fusion and may perhaps be another factor that operates within the attentive phase.[59] Thus the perceptual pattern is not wholly dependent upon conditions at the periphery, the mode and place and nature of the stimulation. Certain more central factors also come into play and justify us in adding to the cross-connections *B-d* and *D-b* the connections b-δ and d-β.

Finally now the perceptual pattern issues in a judgment in accordance with criteria of judgment that have been established. The available factor here lies in the setting up and the preservation of these criteria. Left to himself there is no guarantee of what the subject will do, as the studies of Titchener,[60] deLaski,[61] and Friedline[62] all show. Perhaps the most normal criteria for sophisticated adult subjects would be such that the perceptual δ of our chart would lead to the judgment *Two* and the perceptual β to the judgment *One*. It is a reasonable inference that McDougall's savages in Torres Straits, under the competitive incentive to do well, assumed such criteria that even our β would have led to the judgment *Two* for plainly β is larger than the pattern which a single point most often gives.[63] An overzealous subject can interpret perceptual patterns lying very low in the series as meaning the presence of two points upon the skin; in fact, as Friedline has shown, he can do amazingly well under the influence of both practice and incentive, provided he is not fatigued.[64] On the other hand the sophisticated subjects of the laboratory, and, it may be, therefore the Englishmen, whom McDougall compared with the savages, tend to define *Two* by reference

[57] Henri and Tawney, *Philos. Stud.*, 1895, 11, 394-405, esp. 403ff.; Tawney, *ibid.*, 1897, 13, 186-198; Henri, *Raumwahrnehmungen*, 61-66.

[58] H. Griesbach, *Arch. f. Hygiene*, 1895, 24, 124ff.; but see the summary in C. L. Friedline, The discrimination of cutaneous patterns below the two-point limen, *Am. J. Psychol.*, 1918, 29, 415-418.

[59] Friedline, *op. cit.*, 411f., 418f.

[60] Titchener, On ethnological tests of sensation, *etc.*, *Proc. Am. Philos. Soc.*, 1916, 55, 206-215.

[61] E. deLaski, On perceptive forms below the level of the two-point limen, *Am. J. Psychol.*, 1916, 27, 569-571.

[62] Friedline, *op. cit.*, 405-415, esp. 405f., 408f., 411ff.

[63] W. McDougall, *Rep. Cambridge Anthropol. Expedition to Torres Straits*, 1903, II, 189-193; *cf.* Titchener, *op. cit.*

[64] Friedline, *op. cit.*, 408f., 414f.

to a perceptual pattern higher in the scale.[65] We should hardly have been bold enough to have predicted this result; to the writer, however, it seems reasonable enough now that it has been pointed out. Is it not to be expected that the savage would try to "do well" by discriminating as finely as possible and that the sophisticated person would try to "do well" by discriminating as accurately and consistently as possible, though less finely? In any case the point is that the acceptance of a criterion is an unavoidable experimental condition in determinations of cutaneous spatial sensitivity, and that the criterion must therefore be controlled, since when uncontrolled it gives uncertain significance to the verbal responses *Two* and *One*. In fact it appears that the apparent limen for cutaneous duality may be very much more than quartered by a variation of criterion, and it seems further that the conflicting results in the literature with respect to the effect of fatigue and the effect of practice upon the limen are to be explained in this way.

This leads to a conclusion. If only the end-terms of stimulus and response are controlled a univocal one-to-one correlation between stimulus and response is not possible. In the terms of the diagram, both the stimuli *D* and *B* may condition the response *Two*. There are eight paths leading to *Two*, four from *D* and four from *B*. If we consider the other factors of the schema, the situation is enormously complicated. There are over a hundred modes of connection from *A*, *B*, *C*, *D*, and *E*. to *Two*. Certainly the actual possibilities must be legion. At this level of work the best we can do is to remain in the dark and to deal with relative frequencies, yet relative frequencies do not yield the predictive correlation that science demands. The only way to get out of the dark would be to study the effect of stimulation, of attention, and of criterion by taking hold of these dependent series at their intermediate points, thus providing ourselves with a more complete knowledge and control of the entire psychophysical situation. Now the psychologist of capacity habitually controls stimulation, the various adjustments of the stimulus to the sense-organ, but the psychologist of datum also controls by instruction both attention and criterion. He does not, to be sure, reach the ideal of 100% certainty in the prediction of the response to

[65] McDougall, *op. cit.*, 192 and note; Titchener, *op. cit.*, 211.

a given stimulus, but he is able greatly to increase the precision of these stimulus-and-response correlations. On the other hand, the failure to control the attitudinal factor implied in the acceptance of a criterion, and the attentional factor, again and again results perforce in an equivocal determination of these responses, which is nothing more nor less than a "stimulus-error."

In the psychology of capacity, then, the danger of the "stimulus-error" reduces to the danger that judgments of stimulus will prove scientifically equivocal. Experimentation will show when they are and when they are not. When they are equivocal, the problem is soluble by refinement in the control of conditions. The modern technique for the control of attention and attitude is a method that satisfies scientific standards of accuracy of prediction better than any available substitute. To shut our eyes to this technique in the absence of a substitute would be to refuse to accept scientific methods that have already yielded practical results. Certainly if the psychologist of capacity is to be a successful experimenter, joining hands with the biologist and physicist, he must in some way take account of all the means of experimental control that have been demonstrated as essential to the securing of accuracy.

Summary

Scientific psychology in its inception assumed a distinction between mind and matter and the separate existence of observable mental data and observable physical data. Fechner's psychophysics sought to measure the mental data and to establish their correlation with related physical data. The opponents of this point of view raised the *quantity objection*, arguing that mind is not possessed of magnitude and is therefore not measurable. Most of these objectors were attacking only the quantitative status of psychology and seeking to establish it as an essentially qualitative, but mental, science. Other objectors preferred, however, to keep psychology quantitative by conceiving of it as physical, as the psychology of the *capacity* of the organism for response to stimulus. The older psychology met the quantity objection by showing that the nonexistence of mental magnitude does not preclude mental measurement, and then sought to protect itself against incursions of the physical observational attitude of the psychology

of capacity by styling that attitude the *stimulus-error*. The implication would be that a psychology of capacity does not need to avoid the stimulus-error, but rather should cultivate it. The thesis of this paper is, however, that recent researches have shown that the observational attitude which is directed upon the stimulus—the attitude of the stimulus-error—may sometimes lead to equivocal correlations of stimulus and response which, because equivocal, are unscientific. In the case of the limen of dual impression upon the skin, for example, a psychology of capacity must make use of introspective data if it is to attain its own ideals.

THE ILLUSORY PERCEPTION OF MOVEMENT ON THE SKIN[1]

By Anna Kellman Whitchurch

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The object of the present investigation was to determine the elementary conditions of the perception of cutaneous movement. Wertheimer[2] and his successors have found such conditions for the perception of movement in the field of vision; and Dimmick[3] has shown a specific existential correlate of the perception of visual movement, namely, the grey flash. Our problem, then, is to find out whether the perception of cutaneous movement can be aroused by successive stimulation, and in that case what is its existential correlate.

Benussi and others[4] have already done much work upon the arousal of cutaneous movement by successive stimuli operating at long distances apart. Benussi stimulated at distances ranging from 4 cm. to the distance between the fingers

[1] From the Psychological Laboratory of Cornell University.

[2] M. Wertheimer, Ueber das Sehen von Bewegung, *Zeit. f. Psych.*, 61, 1912, 161ff.

[3] F. L. Dimmick, An Experimental Study of Visual Movement and the Phi Phenomenon, this Journal, 31, 1920, 317ff.

[4] V. Benussi, Kinematohaptische Erscheinungen, *Arch. f. d. ges. Psych.*, 29, 1913, 385; Kinematohaptische Scheinbewegung und Auffassungsumformung, *Ber. ü. d. VI Kong. f. exp. Psych.*, 1914, 31; Versuche zur Analyse taktil erweckter Scheinbewegungen, *Arch. f. d. ges. Psych.*, 36, 1916, 59. *Cf.* also H. E. Burtt, Tactual Illusions of Movement, *Jour. Exp. Psych.*, 2, 1917, 371.

on the two outstretched arms. Burtt gives reports from stimulations 8 to 16 cm. apart, and his apparatus allowed distances from 2 to 18 cm. These distances, however, favor the arousal of visual associations; and minimal distances are for that and for other reasons to be preferred. Incidental reports by von Frey and Metzner,[5] apparently overlooked by later experimenters, prove that such distances may be employed.

Von Frey and Metzner have given the only reports of work on the skin directly comparable with that of Wertheimer in vision. They were trying to find the best interval in which to distinguish as different the responses of two adjacent pressure spots successively stimulated. They used as stimuli hairs or blunt needles which rested on the skin for 1/9 sec. The intervals between stimulations were 1/9, 1/6, 1/3, 1, 4/3, and 2 sec. Near the end of their paper this statement appears: "When two points are stimulated, the difference of the two stimuli is either recognized as such, or (especially with the shorter intervals) is interpreted as a shift (*Verschiebung*) of the stimulus, as a brushing (*Streichen*) of it over the skin." The only numerical data given show that, with an interval of 4/3 second, 13 out of 29 stimulations aroused, in some form, the perception of movement.

Experimental Procedure

The present work was begun in the summer of 1920. The four *O*'s who served were: Miss C. Braddock (B), graduate scholar in psychology; Dr. K. M. Dallenbach (D), assistant professor of psychology; Dr. L. B. Hoisington (H), assistant professor of psychology; Mrs. A. K. Whitchurch (W), graduate scholar in psychology.[6]

The skin on the left fore-arm of the *O*'s was tested with von Frey hairs of various tension-values until a hair was found which could be felt upon the normal skin, but was too weak to arouse subcutaneous sensations when the skin was lightly etherized. The tension-value of the hair finally selected was 7.9 gr./mm. Two hairs were cut to this standard. They measured in length 40 and 42 mm. respectively.

Five groups of pressure-spots, ranging from 6 to 9 in number, on every *O*'s arm were isolated and stained, so that the same spots could be tested every day. During this preliminary testing one spot was stimulated only once in an hour. The stimulating hair was held 1 mm. above the pressure-spot, and

[5] M. von Frey and R. Metzner, Die Raumschwelle der Haut bei Successivreizung, *Zeit. f. Psych.*, 29 1902, 161ff.

[6] When W observed, B acted as experimenter.

after the "Ready, now" signal was touched to the spot. The *O*'s were asked to report on the intensity, quality, and extensity of the sensation. Two reports were taken when the spots were stimulated singly, and two when one spot was compared with another. The introspections were worked over, and the spots most alike were selected and again reported upon, until the first numbers chosen narrowed down to 3 or 4 in a group. Finally we were able to select two adjacent spots in every group which the *O*'s reported as like the spots in every other group. The selected spots were then permanently tattooed with a tiny glass tube just under the hair. The mark was distinct and lasting. The distances between the spots in the various groups were as follows (in mm.): B (1) 2.5, (2) 3, (3) 2.5, (4) 3, (5) 4; D (1) 5, (2) 3.6, (3) 3.2, (4) 3.8, (5) 3; H (1) 4.7, (2) 3.4, (3) 4.7, (4) 3.1, (5) 2.4; and W (1) 2.8, (2) 5.5, (3) 3.2, (4) 2.1, (5) 3.8.

Manual Experiments

O sat at a low table with his arm in a plaster cast. The adjacent hairs in the five groups were stimulated in turn. The duration of the stimulus was $250 \pm 25\sigma$. The intervals, marked by a silent pendulum, were 750, 1000, and 1250σ.

O worked under two sets of instructions. The 'process' instruction was: "I shall stimulate the skin on your arm. Describe the cutaneous perception aroused in purely psychological terms. The stimulus will be repeated so that you may complete your description." The 'meaning' instruction was: "I shall stimulate the skin on your arm. Characterize the cutaneous perception aroused, fully, in any commonsense terms you wish to use. The stimulus will be repeated so that you may complete your report."

The manual experimenting continued until every *O* had given 50 reports under the 'meaning' and 50 under the 'process' instruction with the 750 and the 1250 intervals. With the 1000 interval, every *O* made 100 reports under each set.

Results of Manual Experiments

The reports fell under headings like those which have been obtained in the work on vision: (1) full movement, in which the stimulus moved from the 1st to the 2nd spot stimulated; (2) bimembral movement, in which the 1st and 2nd stimulus each moved; (3) unimembral movement, in which either the 1st or the 2nd stimulus moved; (4) intramembral movement, in which the stimulus at one or both spots moved

within the area of stimulation; (5) pure phi, in which the movement had no qualitative, intensive, or extensive beginning and ending, but was a uniform 'brushing' over the cutaneous surface.

Table A sums up the results of this preliminary work. The columns headed M show the percentage of reports under the 'meaning' instruction; those headed P the percentage of reports under the 'process' instruction. The intervals are given in sigma.

The reports under the two instructions are closely parallel. Long before the completion of the series the *O*'s had settled into a stable manner of reporting.

The 750 interval is the best of the three for arousing the perception of full movement. Even with this interval, however, the percentages are very low. Since optimal conditions had not been found, and since the lowest interval was the best, we began a new series of experiments with shorter intervals. Henceforth we applied the stimuli by means of apparatus.

Mechanical Stimulation: Series I

The apparatus was an adaptation of Benussi's kinohapt.[7] The intervals and durations of the stimuli were controlled by a Leipsic time-sense apparatus.

Both cutaneous and 'subcutaneous'[8] stimuli were employed. The cutaneous stimuli were two horse-hairs whose tension-value was 4.85 gr./mm., and which measured in length 10 and 9.8 mm. respectively. The subcutaneous stimuli were coarser hairs whose tension-value was 10.25 gr./mm.; they measured 3 and 2.85 cm. respectively. The duration of the stimuli was 150$^{\sigma}$. The intervals of this series were 100, 250, 500, 750, and 1000$^{\sigma}$. The adjacent spots in two groups on every *O*'s arm were now stimulated exclusively.

The cutaneous (C) and subcutaneous (S) stimuli were used alternately as follows: (1) C 100, 12 M-reports on spots *a-b*, 12 P-reports on spots *a'-b'*; (2) S 250, 12 M-reports on spots *a-b*, 12 P-reports on spots *a'-b'*; (3) C 500, 12 M-reports on spots *a-b*, 12 P-reports on spots *a'-b'*; and so forth. At the end of the ten groups which this order necessitated, the experiments were continued by reversing the order. Thus after no. 10 came (11) S 100, 12 P-reports on spots *a-b*, 12 M-reports on spots *a'-b'*. The practice-effect was thus evenly

[7] For a description of the kinohapt, see V. Benussi, *Arch. f. d. ges. Psych.*, 29, 1913, 385.

[8] We use this shorthand term to designate the stimuli which affected both cutaneous and subcutaneous end-organs.

TABLE A

MANUAL EXPERIMENTS: PERCENTAGES

		750		1000		1250	
		M	P	M	P	M	P
Full Movement	B	18	14	18	19	4	2
	D	4	4	10	16	14	2
	H	48	30	27	34	0	0
	W	14	24	23	24	2	2
	Av.	21	18	19.5	23.3	5	1.5
	M.V.	13.5	9	5.5	5.8	4.5	.8
Bimembral Movement	B	4	2	5	0	2	2
	D	0	0	0	0	0	6
	H	4	0	10	8	2	0
	W	2	10	13	15	6	10
	Av.	2.5	3	7	5.8	2.5	4.5
	M.V.	1.5	3.5	4.5	5.8	1.8	3.5
Unimembral Movement	B	12	26	9	21	42	38
	D	12	8	26	11	30	20
	H	34	54	32	34	48	42
	W	56	38	38	30	36	48
	Av.	28.5	31.5	26.3	24	39	37
	M.V.	16.5	14.5	8.8	8	6	8.5
Intramembral Movement	B	22	12	17	7	12	12
	D	14	0	3	6	0	4
	H	2	0	13	4	20	14
	W	10	6	8	6	22	6
	Av.	12	4.5	10.3	5.8	13.5	9
	M.V.	6	4.5	4.8	.9	7.5	4
Phi	B	0	2	1	1	4	0
	D	6	0	10	1	2	2
	H	2	2	0	1	0	0
	W	12	10	3	6	0	2
	Av.	5	3.5	3.5	2.3	1.5	1
	M.V.	4	3.3	3.3	1.9	1.5	1
No Movement	B	44	44	50	52	36	46
	D	64	88	51	66	54	66
	H	10	14	18	19	30	44
	W	6	12	15	19	34	32
	Av.	31	39.5	33.5	39	38.5	47
	M.V.	23	26.5	17	20	7.8	9.5

distributed. Several minutes elapsed betwen the stimulations of the same spot. At the end of every 12 reports *O* took his arm from the cast for a time, in order that there might be no fatigue. Usually *O* made reports on one group of experi-

ments in an hour, and never on more than two. Every *O* made 24 reports under the 'meaning' and 24 under the 'process' instruction for every one of the intervals employed.

Results of Series I

The results of the first series with mechanical stimulation are summarised in Tables B I and B II. The columns M and P show the number of reports of the different types of phenomena given in each group of 24 experiments.

With both cutaneous and subcutaneous stimuli the 100σ interval gives the greatest number of full movements. Next to the full movement in frequency comes the unimembral movement. The number of full movements is too small to indicate optimal conditions, and another series was accordingly planned.

The 'process' and 'meaning' reports run closely parallel, as is shown by the averages of the groups.

TABLE B—I

CUTANEOUS MECHANICAL STIMULATION: SERIES I: ABSOLUTE NUMBERS

		100		250		500		750		1000	
		M	P	M	P	M	P	M	P	M	P
Full Movement	B	9	6	3	4	0	0				
	D	8	10	3	0	0	0				
	H	6	8	7	5	1	0				
	W	13	14	0	0	0	0				
	Av.	9	9.5	3.3	2.3	.3					
	M.V.	2	2.3	1.9	2.3	.4					
Bimembral Movement	B	0	0	0	1	0	3	2	0	1	0
	D	0	0	0	0	0	0	0	0	2	2
	H	7	5	6	6	9	7	0	0	0	0
	W	8	5	5	7	8	8	2	1	0	1
	Av.	3.8	2.5	2.8	3.5	4.3	4.5	1	.3	.8	.8
	M.V.	3.8	2.5	2.8	3	4.3	3	1	.4	.8	.8
Unimembral Movement	B	5	8	6	4	10	8	5	6	4	7
	D	12	12	14	9	5	1	4	3	3	0
	H	10	11	10	11	8	9	9	10	9	8
	W	3	5	13	15	14	14	7	8	7	7
	Av.	7.5	9	10.8	9.8	9.3	8	6.3	6.8	5.8	5.5
	M.V.	3.5	2.5	2.8	3.3	2.8	3.5	1.8	2.3	2.2	2.8
Intramembral Movement	B			1		3	5	13	7	9	7
	D			5		0	0	0	0	1	1
	H			0		4	2	5	3	5	5
	W			2		0	0	3	2	3	2
	Av.			2		1.8	1.8	5.3	3	4.5	3.8
	M.V.			1.5		1.8	1.8	3.8	2	2.5	2.3
No Movement	B	9	8	14	15	11	8	4	11	10	10
	D	3	2	2	15	19	23	20	21	18	21
	H	1	0	1	2	2	6	10	11	10	11
	W	0	0	4	2	2	2	12	13	14	14
	Av.	3.3	2.5	5.3	8.5	8.5	9.8	11.5	14	13	14
	M.V.	2.9	2.8	4.8	7.7	6.6	6.6	4.5	3.5	3	3.5

With the 100 interval, B reported phi in 1 M and 2 P; D in 1 M.

TABLE B—II

SUBCUTANEOUS MECHANICAL STIMULATION: SERIES I: ABSOLUTE NUMBERS

		100		250		500		750		1000	
		M	P	M	P	M	P	M	P	M	P
Full Movement	B	10	13	2	3						
	D	14	10	18	11						
	H	15	13	7	7						
	W	15	15	11	10						
	Av.	13.5	12.8	9.5	7.8						
	M.V.	1.8	1.4	5	2.8						
Bimembral Movement	B	0	1	0	0	3	4	2	0	1	0
	D	2	3	6	8	2	0	2	0	0	0
	H	4	0	4	5	0	1	1	1	0	0
	W	4	6	3	5	6	9	4	2	0	0
	Av.	2.5	2.5	3.3	4.5	2.8	3.5	2.3	.8	.3	
	M.V.	1.5	2	1.8	2.3	1.8	3	.9	.8	.4	
Unimembral Movement	B	5	3	12	11	7	4	8	9	0	4
	D	2	4	0	1	12	20	7	6	2	0
	H	4	8	10	10	10	9	8	11	9	8
	W	5	3	10	8	9	7	13	15	7	4
	Av.	4	4.5	8	7.5	9.5	10	9	10.3	4.5	4
	M.V.	1	1.8	3.3	3.5	1.5	5	2	2.8	3.5	2
Intramembral Movement	B	0	0	0	0	2	3	6	5	3	4
	D	0	2	0	0	2	0	0	0	1	5
	H	0	0	2	1	5	8	5	4	4	5
	W	0	0	0	0	2	1	2	2	12	13
	Av.		.5	.5	.3	2.8	3	3.3	2.8	5	6.8
	M.V.		.8	.8	.8	1	2.3	2.3	1.8	3.5	3.2
No Movement	B	9	7	10	10	12	13	8	10	20	16
	D	5	5	0	4	8	4	15	18	21	19
	H	1	3	1	1	9	6	10	8	11	11
	W	0	0	0	1	7	7	5	5	5	7
	Av.	3.8	3.8	2.8	4	9	7.5	9.5	10.3	14.3	13.3
	M.V.	3.3	2.3	3.6	3	1.5	2.8	3	3.9	6.3	4.3

With the 100 interval, D reported phi in 1 M.

Series II

The intervals chosen for the next series were 25, 50, 75, and 100σ. The arrangement of the groups and the procedure were as before.

The results are summarised in Tables C I and C II. The 100σ interval gives the greatest number of full movements with cutaneous stimulation, and the 75σ interval with subcutaneous stimulation. Again the 'process' and 'meaning' reports parallel each other.

A group of 24 experiments with each stimulus in each of the intervals 125 and 150σ was made as a part of this series with *O*'s H and W. Full movements were reported in the following cases: C 125, H 9M and 8P, W 8M and 6P; C 150, H 5M and 4P, W 4M and 4P; S 125, H 6M and 4P, W 5M and 3P; S 150, H 6M and 6P, W 5M and 4P.

TABLE C—I

CUTANEOUS MECHANICAL STIMULATION: SERIES II: ABSOLUTE NUMBERS

		25		50		75		100	
		M	P	M	P	M	P	M	P
Full Movement	B	0	0	19	23	17	16	14	19
	D	5	1	9	6	7	8	11	9
	H	2	6	10	4	11	11	14	15
	W	0	0	10	13	14	15	16	14
	Av.	1.8	1.8	12	11.5	12.3	12.5	13.8	14.3
	M.V.	1.8	2.1	3.5	6.5	3.3	3	1.4	2.8
Bimembral Movement	B			0	0	0	0	0	0
	D			0	0	0	0	0	0
	H			4	4	2	3	4	3
	W			2	0	4	3	4	5
	Av.			1.5	1	1.5	1.5	2	2
	M.V.			1.5	1.5	1.5	1.5	2	2
Unimembral Movement	B	1	0	0	0	0	3	2	2
	D	12	8	12	14	11	6	10	9
	H	5	5	9	11	7	7	6	6
	W	1	1	3	4	6	6	4	5
	Av.	4.8	3.5	6	7.3	6	5.5	5.5	5.5
	M.V.	3.8	3	4.5	5.3	3	1.3	2.5	2
Intramembral Movement	B	3	4	3	0	2	0	2	0
	D	0	0	0	0	0	0	0	0
	H	0	0	0	0	0	1	0	0
	W	2	0	0	0	0	0	0	0
	Av.	1.3	1	.8		.5	.3	.5	
	M.V.	1.3	1.5	1.1		.8	.4	.8	
No Movement	B	20	20	1	1	5	5	6	3
	D	7	14	2	4	6	10	3	6
	H	17	13	1	5	4	2	0	0
	W	21	23	9	7	0	0	0	0
	Av.	16.3	17.5	3.3	4.3	3.8	4.3	2.3	2.3
	M.V.	4.3	4	2.9	1.8	1.9	3.3	2.3	2.3

With the 25 interval, D reported phi in 1 P; with the 50 interval, B reported it in 1 M, and D in 1 M.

Types of Perception

The reports arising from the cutaneous and subcutaneous stimulations are so nearly alike that we shall for the most part confine our discussion to the former and shall mention the 'subcutaneous' reports only where differences occur. The reports under the 'meaning' and 'process' instructions will not be treated separately. They will be grouped together in the following discussions, and throughout 'meaning' will be dealt with first.

(1) *Full Movement.*—In all cases of full movement, the stimuli are felt as two touches with movement between them from *a* to *b*. The type appearing most often is that of the dumb-bell pattern. The stimulus touches, lifts, and moves in a narrow line to a new position. In 'process' the first impression is a round or oval bit of extent. The impression

TABLE C—II

SUBCUTANEOUS MECHANICAL STIMULATION: SERIES II: ABSOLUTE NUMBERS

		25		50		75		100	
		M	P	M	P	M	P	M	P
Full Movement	B	1	5	5	2	11	14	15	17
	D	7	6	6	1	18	12	11	15
	H	0	0	3	3	16	15	10	10
	W	0	0	2	0	19	18	17	15
	Av.	2	2.8	4	1.5	16	14.8	13.3	14.3
	M.V.	2.5	2.8	1.5	1	2.5	1.8	2.5	2.1
Bimembral Movement	B			0	0	0	0	0	0
	D			0	0	0	0	0	0
	H			0	2	0	2	3	1
	W			0	0	1	2	1	0
	Av.				.5	.3	1	1	.3
	M.V.				.8	.4	1	1	.4
Unimembral Movement	B	0	0	1	6	1	5	0	0
	D	11	13	16	21	0	0	9	2
	H	2	1	9	9	6	7	11	13
	W	4	2	4	4	4	4	6	9
	Av.	4.3	4	7.5	10	2.8	4	6.5	6
	M.V.	3.8	4.5	5	5.5	2.3	2	3.5	5
Intramembral Movement	B	7	0	2	3	2	1	1	2
	D	0	0	0	0	1	0	0	0
	H	0	1	3	1	0	0	0	0
	W	0	0	0	0	0	0	0	0
	Av.	1.8	.3	1.3	1	.8	.3	.3	.5
	M.V.	2.6	.4	1.3	1	.8	.4	.4	.8
No Movement	B	16	19	16	13	10	4	8	5
	D	6	5	2	2	5	12	3	7
	H	22	22	9	9	2	0	0	0
	W	20	22	18	20	0	0	0	0
	Av.	16	17	11.3	11	4.3	4	2.8	3
	M.V.	5	6	5.8	5.5	3.3	4	3.9	3

With the 100 interval, D reported phi in 1 M.

extends in time into another oval or circular bit of experience like the first. The following reports are typical:

H (C 100 M) The hair touched lightly and began moving at once. As it moved it pressed more lightly and almost lifted off. Then, as it moved on, it pressed harder, and came down fairly lightly at the end. It moved faster at the beginning than it did in the middle.

H (C 100 P) The process began limited in extent. It increased in one direction to fairly extended, with slight contraction ending in a little spread of extent. It began as neutral pressure, becoming increasingly like contact until it was almost pure contact, with a rapid shift to pure neutral pressure. It was fairly weak, diminishing in intensity during the extent of the process, and becoming more intense at the end. The whole was very short.

The next type is much like the dumb-bell pattern, save that the movement does not start from the center of the first touch, but rather begins at the edge. In 'process' the first impression is described as static. Then there follows a growth from the edge.

H (C 100 M) A hair touched and rested; and then something started to move from the edge of the first touch and moved a little way, rested, and lifted off.

H (C 100 P) The process was limited and static for a bit; then growth began from the edge and became fairly extended. At first it was a dull neutral pressure, quickly shifting to a brighter pressure, and becoming neutral at the end. It was fairly weak, and dropped to weak. It was of short duration.

D (C 100 P) The process began as a contact fused with a wiry prick, immediately followed by a vague, diffuse, large pressure sensation localized differently.

In a third type the touches at beginning and end are of the same size as the moving touch between. There is no narrowing during the movement. In 'process' the terminals are marked by differences in quality and intensity, but not by a spread of extent. The first may be a pressure rather intense, merging into a spreading that is less intense, but of continuous extent, and ending in another intense pressure of the same extent.

W (C 100 M) A touch moved smoothly across my arm. It pressed harder at the beginning and ending than it pressed in the middle of its course. It was the same breadth all the way.

W (C 100 P) The process began, limited in extent, and spread in time, becoming weak during the spread but not contracting, and increasing in intensity as it became more extended. It began as good contact, and remained contact throughout. There was no change in extent at the terminals. The whole was short.

D (C 100 P) The process was neutral pressure, the area of which seemed to expand to another neutral pressure.

B (C 100 P) The process was a contact changing in intensity along a line.

The *O*'s describe the extension of the process in various other ways. It is called a 'flowing,' an 'extending in time,' a 'diffusing in time.' At first the word 'flow' was frequently used, but during the later reports it does not appear. Two *O*'s have said that 'flow' does not describe truly the depth of the sensation. Even when the intensity is weak, the growth seems to have the feel of something deeper than 'flow.' 'Ooze' has sometimes been used. H usually described the process as 'growth;' B and W used the term 'spread' most frequently; D spoke of 'extension and diffusion,' 'sensory continuum in time,' 'progression in time.'

With cutaneous stimuli the terminals, with few exceptions, are contact or neutral pressure. The same terms usually describe the subcutaneous experience; occasionally this is called 'drag' in the reports of H and W. In a few cases the subcutaneous stimuli produced a 'thicker' feel in the spread than did the cutaneous. In nearly all cases, except

those just mentioned, the reports for the two stimuli are exactly alike.

(2) *Full Movement in an Arc.*—Occasionally the *O*'s reported a full movement in the shape of an arc. The first touch became larger in some one direction, and at the coming of the second it seemed to be pulled round to that position. In 'process' this type is described as a pressure becoming extended irregularly, and spreading around one side to another impression.

D (C 75 M) A touch went around in an arc to a new position.
B (C 100 M) The hair moved on my arm in the form of an arc, and rested in another place.
H (C 100 P) The process began limited and spread in extent out around one side, becoming narrower and ending in a slightly greater extent. It was neutral pressure, then contact with a shift to neutral pressure. It was fairly weak, decreasing in intensity during the extent, and increasing to fairly weak at the end. It was of short duration.
B (C 100 P) A contact diffused in a half circle and fused with another contact.

No *O* reported the arc movement more than five times in all the experiments performed. It occurred only when full movements were reported. Unimembral and bimembral movements did not take the arc form.

(3) *Bimembral Movement.*—In the bimembral movement the stimuli are felt as two touches which move, but which have a spatial and temporal interval between them. In nearly all cases *a* moves toward *b*, and *b* moves away from *a* to a resting place. Occasionally *b* touches and then moves on and off without resting. In 'process' the experience is described as two pressures or contacts, separated spatially and temporally, both diffusing in time.

B (C 500 M) A hair touched me and moved a bit. It lifted off, and came down moving into a new place.
H (C 100 P) The first process began very limited and grew in extent to slightly extended. It was a neutral pressure quality fading out, and it was very short. The second process was limited and shrank in time to very limited. It was neutral pressure with a hint of contact at the beginning, and it was weak and short.
D (C 100 P) Two neutral pressures occurred successively in time and were localized differently in space. The first became diffused in time, and the second likewise.

(4) *Unimembral Movement.*—In the unimembral movement, sometimes the first stimulus moved and the second was stationary, but more often the second was the moving member. Stimulus *a* touched and was gone; *b* touched while moving away from *a* to a new position. In 'process' one of the impressions was said to spread or extend in time.

H (C 100 M) A hair touched me fairly lightly and lifted off. It touched again in another place, moving a short distance. It rested and lifted off.

W (C 100 P) First there was a contact limited and very weak. Near it in space was a contact spreading in time to a larger extent and becoming more intense during the spread.

H (C 100 P) The process began limited, and grew slightly in extent; it was dull neutral pressure becoming pure pressure in the extent. It was fairly weak, with the intensity dropping off. It was very short. The second process was very limited neutral pressure, and was fairly weak and short.

There were other reports more rarely given. Sometimes only one stimulus was felt, and it moved to a stopping place, or began as static and moved off. In 'process' such experiences were called "a spreading contact growing more extended and more intense as it spread," or "a contact which spread and faded out."

(5) *Intramembral Movement.*—The intramembral movement is "the feeling of movement as if the hair were held in an unsteady hand," or "a wiggling of the hair against the skin." In 'process' the experience is described as "a slight shift of the intensive pattern in time."

D (C 250 M) There were two touches successive in time and localized differently in space. The first quivered as if held in an unsteady hand.

B (C 500 P) There were two contacts differently localized and successive in time. The second had changes of intensity in time; otherwise it was just two.

W (C 1000 P) The first process was a contact which fluctuated in intensity in the touched area. The second process was pure contact, very limited and short.

H (C 100 P) The first process was very limited and rather a brightish pressure of moderate intensity and very short. The second process was a fairly limited neutral pressure quality. There was a shift in time of the intensive patterns from moderate to fairly weak. The whole was short.

(6) *Intramembral plus Unimembral Movement.*—In the manual experiments and the first series of the mechanical stimulations there were reported some cases of combined perceptions of intramembral and unimembral movement. Sometimes one stimulus gave the perception of intramembral movement, and the other that of unimembral movement. Occasionally one stimulus would begin its movement as intramembral, and then extend into unimembral movement. These reports do not appear in the last series, in which the reports of unimembral and intramembral movements became uniform and clear-cut, with no hint of confusion or addition.

(7) *Phi Movement.*—The phi movement occurs infrequently. A typical 'meaning' report is: "It felt as if a moving some-

thing flicked against my arm." In this perception we do not have the feeling of two touches, or of one touch moving to a new position. In 'process' there were no distinct terminal experiences; the pressure is called "a weak, spreading contact, a bit more intense in its middle portion." Most of the pure phi-reports occurred in the manual experiments, and came early in the series.

(8) *Backward Movement.*—In the manual experiments and in the first series with the mechanical stimulations there were a few reports of unimembral and bimembral movement in which the stimulus seemed to move backward. That is, one impression was felt; and the second, instead of moving away from it to a new position, would be felt some distance away moving toward the first. H and W reported this perception three or four times in the manual experiments, but not afterwards. B reported it twice in the manual experiments; four times in Series I with the mechanical stimulations, and not at all in Series II. D reported backward movement three times in the manual experiments; in Series I of the mechanical stimulations he gave no such reports; in Series II he reported it 24 times (6.25% of all the reports in that series).[9]

(9) *No Movement: Two Points Distinct.*—In nearly all cases in which no-movement was reported, the perception was of two points touched successively in time and having spatial separation. Sometimes the one impression was more diffuse than the other, or differed from it slightly in quality; usually the two were reported as alike. This type of perception occurred in all experiments with intervals above 50σ.

(10) *No Movement: Conjunctive.*—A report made frequently with the 25σ intervals belongs to the no-movement group, though it indicates something more than merely two touches which did not move. The second stimulation was added to the first, or placed beside it. Sometimes the second grew larger after it had appeared, and again it came as a diffuse, ill-defined touch. In 'process' one contact or pressure had added to it another, and there was little separation of the two in space and time.

D (C 25 M) There were two light touches which came separately, but whose areas overlapped.

H (C 25 P) The first process was very limited. It was a weak contact and very short. Beside it came another process of pressure

[9] D's observation of backward movement evidently became a matter of habituation. Very occasionally in this series he reported triple pressure; as, for instance, pressure at a followed by a pressure at b which moved back toward a and then ended with increased intensity.

quality, with a fringe having in it a hint of contact. It was of very weak intensity and very short.

H (C 25 P) The process was very limited and was added to. It became fairly limited in one dimension. It was of neutral pressure quality with a fringe of contacty pressure. It was fairly weak. The whole process was fairly extended and fairly weak.

(11) *No Movement: Superpositive.*—Occasionally the two stimulations seemed to be successive in time but to fall upon the same spot.

W (C 25 M) There were two touches, first one and then another beside it covering it. The two were almost successive in time.

H (C 25 P) The first process was very limited, and while it was still present another limited process covered it. The second was more extended, but there was perfect fusion of the two. Both were good neutral pressure quality, of weak intensity. The first was very, very short. The second was slightly longer.

D (C 25 P) The first process seemed obscured by the second. I was conscious of the first, but it was absorbed by the second, which was big and which gradually became bigger.

It will be noted that whereas B, H and W have a large number of reports falling into the no-movement group in the 25σ interval, D has but few.[10]

Distance of Movement

Under the 'process' instruction the *O*'s never reported how far the stimulus moved in full movement, or how far apart were the members in part movement. In 'meaning' the perceptive distance in the larger intervals, as occasionally reported, was always greater than it was in reality, and varied according to the interval. With the 1000σ interval it was reported as 1.5 to 2 cm. As the intervals shortened, the distance decreased to 4 or 5 mm. at the 100σ interval, and at the 25σ interval the stimulations were side by side.

Rate of Movement

Rate of movement, like distance, was reported only under the 'meaning' set. Sometimes during the manual experiments the *O*'s said that the movement went "quick as a flash" or "rapidly." Sometimes in the full movement reports one part of the movement was said to move more quickly than another. In bimembral movements, the *O*'s occasionally said that "the second went quicker than the first." Aside from instances where one part of a movement could be compared with another in the same perception, the *O* was uncertain of the rate, and hesitated to report it. During the mechanical

[10] See footnote 9.

stimulations the reports of rate dropped out. If questioned about it, the *O*'s said: "I am not sure," or "I can't report on it."

Stimulations of Long and Short Duration

After the series with the duration of the stimulus at 150σ, four other series (2C and 2S) of 24 experiments each were performed with H and W. In these series the 75 and 100 intervals were used, and the durations were 75 and 200σ. No full movements were reported by W with duration at 200σ; H gave such reports in 6M and 4P at C 100. With duration at 75σ with C 100, H reported full movement in 14M and 17P, and W in 14M and 15P. These reports are almost the same in number and content as those with the duration at 150σ. With C 75, H had no full movements and W had 5M and 8P. With S 75 neither H nor W reported full movement. With S 100, H had 8M and 2P full movements, and W had 8M and 8P. Since the intervals longer and shorter than 150 proved to be not so good as 150, and in only one case compared favorably with it, no farther experiments were performed with them.

Experiments Upon Non-Adjacent Spots

A short series of 24 experiments with each stimulus was performed upon non-adjacent spots. The spots *a-c* and *a'-c'* were used. H and W again served as *O*'s. The stimulations were made with the 75 and 100σ intervals. The duration was 150σ. H reported no movement during any of the experiments. W in S 75 had 8M and 12P unimembral movements. The movement always occurred with the second member. With C 100, W reported 12M and 14P unimembral movements, also at the second member. The movements were very short. There was just a hint that the second stimulus slid a very short distance into place. In 'process' the second stimulus had a very slight diffusion in time.

Cutaneous Versus Visual Movement

Dimmick under his best conditions obtained full movement with 'process' instructions in 90 to 93% of his reports, and with 'meaning' instructions in 81 to 91%. We under the most favorable conditions obtained on the average with C only 13.75 (57%) M-reports and 14.25 (59%) P-reports out of a possible 24; and with S, 16 (67%) M and 14.75 (61%) P. It seems from a comparison of these figures that the cutaneous perception of movement is less fundamental and

compulsory than the visual. There was, it is true, a difference in the plan of the two investigations, since Dimmick's stimulus *a* in his regular series always bore a fixed spatial relation to his stimulus *b;* whereas the spatial relations of the spots stimulated by us varied from group to group. Our stimulus *b* might lie above or below *a*, to right or to left of *a*. It is difficult to say how far this difference influenced the results; but it seems from what indications we have that the influence was slight. (1) A special C-series made with H, in which the same two spots *a-b* were stimulated in the same order in successive observations to the point of exhaustion, yielded no increased percentage of full movements. (2) The conditions of our mechanical stimulation approximated those of Dimmick's experiments. After the first two or three stimulations of two spots at the beginning of a group of twelve experiments, *O* knew when the second stimulation came that it was in the same place as before, and so learned to expect it at that place. H, who observed in Dimmick's work, said: "I was not conscious of expecting the second stimulation in the place where it came, but when it did come I had the feeling that it was where it ought to be and where it had been before." H further remarked that the expectancy was "the same sort of thing as in Dimmick's experiments." W, who also observed with Dimmick, verifies these reports.

Only one *O*, B, reported associated visual images, and she only in the early stages of practice. H and W declared that visual images did not appear; and D when questioned said that he had them occasionally, but that they were never a determining factor in his perceptions.

The aberrant types of perception, the arc-movement and the backward movement, seem to be conditioned upon some directional emphasis within the tactual spread. Ordinarily the spread of sensation is roughly circular or oval. If, however, some lateral segment of *a*'s spread is unusually intensive, the arc movement necessarily follows; and if *a*'s spread is uniformly weak, and *b*'s is strongest over a segment that points toward *a*, the perception of backward movement follows. We have no means of knowing how these local emphases arise, but the *O*'s reports show that they appeared and formed the basis of judgment. There is, of course, nothing parallel in Dimmick's results.[11]

[11] Dimmick tried to get backward movement by artificial emphasis and failed: *op. cit.*, 331.

Conclusions

(1) We have succeeded in synthetizing the perception of cutaneous movement, from two separate and successive stimulations, under conditions that parallel Wertheimer's synthesis of visual movement.

(2) The optimal conditions for the arousal of the perception are: with cutaneous stimuli, a duration of 150σ and an interval of 100σ; with stimuli that also affect the subcutaneous organs, a duration of 150σ and an interval of 75σ.

(3) (*a*) The most important conditions of the perception, under our conditions, are the interval elapsing between the successive stimuli and the adjacency of the spots stimulated. (*b*) The duration of 150σ is best for arousing the perception of full movement; the number of full movements aroused with longer times is much lower. The same thing is true also of the duration of 75σ except in one instance, namely C 100, where the number of full movements aroused compared favorably with those reported when the duration was 150σ. (*c*) The stronger of our two intensities, the 'subcutaneous' stimulus, yields a slightly larger percentage of full movements with its best interval, 75σ, than does the cutaneous stimulus at its best interval, 100σ.

(4) Whereas Dimmick was able to secure the perception of visual movement, under optimal conditions, in practically 100% of his trials, we found that the perception of cutaneous movement appeared, under the same conditions, in only 57 to 67% of our observations. Since, so far as we can discover, there is nothing in the arrangement of the two experiments that accounts for this difference of result, we are forced to the conclusion that the cutaneous perception is less fundamental and compulsory (in Dimmick's sense) than the corresponding visual perception.

(5) Support for this view is found in a comparison of the existential correlates of the two perceptions. Dimmick's grey flash is an integration of quality with time; space is not involved. The existential correlate of the cutaneous perception appears, on the other hand, to be an integration of quality, time and cutaneous extent. While Dimmick's *O*'s reported a curtain or film of shimmering, liquid, live grey, our *O*'s (in part the same as Dimmick's) report a pressure diffusing, growing, extending, in time. The integration appears to be of a higher order, and therefore less stable and inevitable.

(6) We regard it as entirely premature to theorise from the results of Wertheimer and Dimmick in vision and from our own results in touch to 'the' perception of visual or cutaneous movement. It may very well be that the singular number must be replaced in this and similar connections by the plural. 'Movement' is, after all, a gross meaning that may be carried by several existential correlates; or, to put the same thing in other words, an organism whose outfit of sense-organs is as complex as our own may be adequate to the 'perception of movement' by way of several different primary integrations.

SOME QUALITATIVE ASPECTS OF BITONAL COMPLEXES[1]

By Carroll C. Pratt

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The present investigation neither pretends to a resolution of the problem of fusion in its systematic context nor to a new determination of the order of degrees of fusion. It was initiated under the conviction that there were within reach a point of view and a method, which would single out for separate examination certain phenomenological characteristics which enter into the total impression set up by two tones when presented simultaneously to the organ of hearing. The literature on tonal fusions, although rich in experimental studies undertaken from various angles of approach, is nevertheless far from being in a settled state. The suspicion is hard to escape that much confusion has resulted from a failure to take into consideration the fact that any judgment upon the nature of fusion is inevitably ambiguous unless directed upon one univocal characteristic of the tonal impression.

A review of the literature would be unnecessary iterance in the present article.[2] Many of the controversies to which Stumpf's classic volumes gave rise are due to Stumpf's ambiguity of definition and procedure, or at best, to a misunderstanding on the part of others of his exact concept of fusion. It is not surprising that misunderstandings should arise. As a rule Stumpf displays a predilection for calling fusion "dasjenige Verhältnis zweier Inhalte, speciell Empfingdungsinhalte, wonach sie nicht eine blosse Summe sondern ein Ganzes bilden."[3] Nevertheless by way of amplification, Stumpf refers to fusion as "dasjenige Verhältnis zweier Empfindungen, in folge dessen" the total impression of the more perfect fusions "immer mehr dem Einer Empfindung nähert und immer schwerer analysirt wird."[4] Of

[1] From the Psychological Laboratory of Clark University.

[2] For a recent review of the literature, see W. Kemp, Methodisches und Experimentelles zur Lehre von der Tonverschmelzung, *Arch. f. d. ges. Psychol.*, 1913, 29, 139-181.

[3] C. Stumpf, *Tonpsychologie*, 1890, ii, 128.

[4] *Loc. cit.*

course such aspects of fusion are secondary, but Stumpf uses them in his experiments as criteria for fusion. Again Stumpf calls fusion "eine unveränderliche Eigentümlichkeit des Empfindungsmateriales, welche immer noch übrig bleibt, wenn alle anderen Hindernisse der Analyse beseitigt werden, und welche gerade nachdem die Analyse vollzogen und die Töne deutlich als zwei erkannt sind, ebenfalls erst in sich selbst bemerkt werden kann."[5] "Wie sich dies ausnimmt, muss man eben hören und kann es Niemand klarmachen, der nicht hören oder Gehörserscheinungen nicht beobachten kann."[6] Stumpf throws in another confusing implication when he remarks that "der Zusammenklang zweier Töne nähert sich bald mehr, bald weniger dem Eindruck Eines Tones, und es zeigt sich, dass dies um so mehr der Fall ist, je konsonanter das Intervall ist."[7] Wundt[8] has brought the reproach against Stumpf of creating a purely logical concept of fusion, in spite of Stumpf's manifest efforts to base his inductions on phenomenological experience. Others,[9] accepting the Stumpfian doctrine on its theoretical side, or, on the contrary, dispensing with it, or at times modifying it, have centered their interest in the search by repeated experiment and observation for a more stable order of degrees of fusion. Still others[10] doubt the justification for introducing a unique notion like fusion into the psychological system and attempt to account for the phenomenon in terms of analysability, unitariness, harmony or what not, or else to work out an entirely new definition of fusion.

Kemp's experiments are an attempt to separate out the factors which may enter into the phenomenon of consonance. The results indicate the operation of the following four *Merkmale:* fusion in the sense of that "unveränderliche Eigentümlichkeit welche immer noch übrig bleibt, wenn alle anderen Hindernisse der Analyse beseitigt werden;" pleasantness and unpleasantness; *sinnlicher Wohlklang; sinnliches Zusammenpassen,* a sort of relationship by virtue of which the components of an interval seem to belong or go together; and *harmonisches Zusammenpassen,* the need of resolution (*Auflösungsbedürfniss*).[11] Unfortunately, however, Kemp gives no clue as to the structural na-

[5] *Ibid.*, 127f.

[6] *Konsonanz und Dissonanz,* 1898 44.

[7] *Ibid.*, 35.

[8] *Physiologische Psychologie,* 1910, 6 Aufl., ii, 124ff.

[9] E.g., O. Külpe, *Grundriss der Psychologie,* 1893, 289ff.; A. Faist, Versuche über Tonverschmelzung, *Zeits. f. Psychol.*, 1897, 15, 102ff; Th. Lipps, *Leitfaden der Psychologie,* 1909, 3 Aufl., 98ff; Tonverwandschaft und Tonverschmelzung, *Zeits. f. Psychol.*, 1898, 19, 1ff; W. Kemp, *op. cit.*, 153; T. H. Pear, The experimental examination of some differences between the major and minor chords, *Brit. J. Psychol.*, 1911, 4, 56ff.

[10] Wundt, *Phys. Psychol.*, 1910, ii, 121; F. Krueger, Beobachtungen an Zweiklängen, *Philos. Stud.*, 1900, 16, 307ff; Die Theorie der Konsonanz, *Psychol. Stud.*, 1906, 1, 305ff; 1907, 2, 205ff; 1909, 4, 201ff; 1910, 5, 294ff; P. Natorp, *Göttinger, Gel. Anz.*, 1886, 145ff; 1891, 789ff; H. Cornelius, Ueber Verschmelzung und Analyse, *Vierteljahrsschr. f. wiss. Philos.*, 1892, 16, 404ff; E. Buch, Ueber die Verschmelzung von Empfindungen, besonders bei Klangeindrücken, *Philos. Stud.*, 1899, 15, 1ff, 183ff.

[11] Kemp, *op. cit.*, 181-203.

ture of these *Merkmale:* an omission regrettably common in work on the psychology of fusion.

The present experimental investigation was begun in the fall of 1919 and continued until the spring of 1921 in the Psychological Laboratory of Clark University.

Preliminary Series

The *O*'s who took part in the experiment were G, Dr. E. G. Boring, who worked with knowledge of the problem; F, Dr. S. W. Fernberger; L, Dr. L. D. Boring; Y, Dr. M. Yokoyama; and B, Miss M. Bates. All were highly trained in observation. B was the most "musical" *O*, but found no difficulty in abstracting from musical knowledge. L had absolute recognition of single pitches but had no musical recognition of intervals. Y had but slight "ear" for occidental music and could not recognize intervals musically at all. G and F had no musical recognition of intervals.

Three Stern variators (210-400, 310-600, and 410-800 d.v.) were used as stimuli throughout the experiments. They were placed side by side on a wooden support 80 cm. from the floor on one side of the experimenting room. *O* sat with his back toward the source of sound at a distance of 1.5 m.

Preliminary experiments were conducted to determine roughly the rank order of degrees of fusion for the five *O*'s on the basis of rapidity of analysis, a method suggested by Külpe.[12] A Bergström chronoscope, placed alongside the variators and connected with the reaction key on the arm of *O*'s chair, was used to measure reaction times. The octave, fifth, fourth, sixths, thirds, tritone, sevenths, and seconds were used. The order of presentation was varied from time to time, and occasional *Vexirversuche* of single tones were thrown in. Fifty reactions upon each interval were taken.

The following typewritten instructions were read by *O* at the beginning of each session: "Following the 'ready' signal, you will be presented with (*a*) single notes, and (*b*) two-note complexes. The duration of the stimulus will be 0.5 sec. You are requested in each case to react upon the key as soon as you have decided whether the sound which you hear is that of one note or two notes, stating afterwards whether the judgment was 'one,' 'two,' or 'doubtful.' In case of the latter, indicate, if possible, the direction in which the judgment inclined."

Proceeding on the uncritical assumption that degrees of fusion are inversely proportional to rapidity of analysis, a scale of degrees of fusion was calculated from the reaction times of each *O*. The rank orders thus obtained were utilized in the next step of the preliminary experiments.

The purpose in this second part of the preliminary experiments was to present tonal complexes to *O* which could not readily be analyzed into separate components and to secure from *O* qualitative descriptions of the impressions thus set up. Although Stumpf[13] insists that the phenomenon of fusion still persists after analysis is complete, it was deemed worth while to forestall analysis at least in part by combining the intervals which had yielded less quickly to analysis in the preceding experiments into triads that could be but very imperfectly separated. (*E.g.*, the first six intervals which resulted from the foregoing experiments for L were in order of difficulty of analysis: octave,

[12] *Grundriss der Psychologie*, 1893, 239.
[13] *Tonpsychologie*, ii, 127f.

fifth, fourth, major third, minor sixth, major sixth. These intervals were combined into the following triads: d-g^{b}-a, major third and fifth; c-g-c, fifth and octave; d-g-d, fourth and octave; d^{b}-f-b, major third and major sixth; a-f-a, minor sixth and octave; b-a^{b}-b, major sixth and octave; d-g-b, fourth and major sixth; and c-e-c, major third and octave. Similarly for the other *O*'s.)

In these experiments the stimuli were allowed to sound for one sec. *Vexirversuche* of two-note complexes were occasionally interspersed. The instructions were similar to the previous experiments, except that the phrases "two-note" and "three-note complexes" were substituted for "single notes" and "two-note complexes." It was not long before the judgments and the length of reaction times furnished *E* with sure indication that certain of the triads were submitting very imperfectly to analysis. Judgments of "two" for three-note complexes were very frequent; or the time of reaction was often so great that *E* lost count of the number of swings of the pendulum across the 2000σ scale. *E* secured introspective descriptions of the impressions aroused by these more imperfectly analyzable triads. The qualitative data thus obtained put *E* in the possession of valuable descriptive material for experiments to be mentioned presently.

It now appeared desirable to adopt a direct method of observation.[14] The method of paired comparisons provided a suitable form of procedure. Every one of the twelve intervals was paired with the eleven other intervals six times, making thus $(12 \times 11)/2$ comparisons x 6 repetitions = 396 judgments in all. There were 11 comparisons x 6 repetitions = 66 total possible preferences for any one interval. The intervals were arranged in haphazard order, with the precaution that no interval should be presented twice in succession. The components for the intervals were all included within the range of a major 9th, from b^{b} of the small octave (234.66 d.v.) to c^{2} of the twice-accented octave (528 d.v.). With an eye to the time-error, the order of presentation of the intervals within a pair was reversed in alternate series. It was possible to secure about 100 judgments in each experimental session.

The following instructions, derived from the Stumpfian *Folgeerscheinung* of *Einheitlichkeit*, were read by *O* at the beginning of every session: "Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of unitariness, regardless of other possible characteristics of the impressions. Report 'first' or 'second' according as the first or second impression is more unitary. If there is no difference between them, report 'same.'"

These instructions proved to be highly equivocal for all *O*'s, for they soon discovered that the meaning of unitariness might be carried by quite different sensory data present in the total tonal impression. The judgments were made only with extraordinary difficulty and reported with still greater reluctance, for *O* was peculiarly loth to trust his judgment when faced with several processes all of which were possessed of potential contexts for unitariness ih nearly equal degrees of mediacy.

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[14] On direct *vs.* indirect methods in work on tonal fusions, see Kemp, *op. cit.*, 163-179.

The situation thus created is interesting in the light of recent attention to the "stimulus-error" in psychophysics.[15] Ordinarily a careful control of attitude is necessary to secure univocal determination in psychological experiment. The experiments of George[16] indicate the ease with which equivocal criteria may arise with inconstancy of attitude, and the recent work of Friedländer[17] and Fernberger[18] with lifted weights makes clear the effect of this inconstancy upon the objective results which psychometric functions represent. The manner in which the limens for dual impression upon the skin may vary under attitudinal shifts has become well established in cutaneous psychophysics.[19] This development simply serves to give impetus to the current urge in experimental work for a sharper delineation of observational attitudes, to the end that a more univocal correlation may be arrived at between the terms in the total chain of experience. This chain, following Urban's[20] schema, may be represented by $\alpha, \beta, \gamma, \ldots$ for physical conditions (stimulus-situation), $a, b, c, \ldots$ for physiological processes of nerve excitation, and $A, B, C, \ldots$ for contents of consciousness. Since similar terms in these three systems, as β, b, B, are not in experimental practice univocally correlated, it becomes necessary to discover what factors in the $A, B, C, \ldots$ system are correlated, e. g., with β in the stimulus-situation; and it may turn out that the conscious correlates of β are A, C, M, and P, depending on the nature of the *Aufgabe* which is operative in the stimulus-situation. If the Aufgabe is not set carefully by instructional determinants, O may judge now upon one, now upon another, of the A, C, M, P, factors, so that it becomes scientifically absurd to speak of a correlation. Hence "the total *Aufgabe* under which the subject judges must be made definite in instructions, and must be more fully determined by means of repeated characterizations by the subject of his attitude and procedure. This latter check is important since much of the subject's instruction is apt to be a self-instruction."[21]

In the series of experiments under consideration, it became apparent that the O's were laboring under an equivocal instructional determinant. Let it be supposed, for instance, that the tonal intervals in the stimulus-situation had among other conscious correlates the factors B, D, L, and Q, all of which were potentially associated with the contexts B', D', L', and Q', carrying the meaning for unitariness.

[15] See especially E. G. Boring, The control of attitude in psychophysical experiments, *Psychol. Rev.*, 1920, 27, 446ff., 449; The stimulus-error, *Amer. J. Psychol.*, 1921, 32, 449ff.

[16] S. S. George, Attitude in relation to the psychophysical judgment, *Amer. J. Psychol.*, 1917, 28, 1ff.

[17] H. Friedländer, Die Wahrnehmung der Schwere, *Zeits. f. Psychol.*, 1920, 83, 129ff.

[18] S. W. Fernberger, An experimental study of the "stimulus-error," *J. Exp. Psychol.*, 1921, 3, 63ff.

[19] E. B. Titchener, Ethnological tests of sensation and perception, *Proc. Amer. Philos. Soc.*, 1916, 55, 206-215; E. deLaski, Perceptive forms below the level of the two-point limen, *Amer. J. Psychol.*, 1916, 27, 569-571; C. L. Friedline, Discrimination of two cutaneous patterns below the two-point limen, *ibid.*, 1918, 29, 400-419; and especially Boring, The stimulus-error, *Amer. J. Psychol.*, 1921, 32, 449ff.

[20] F. M. Urban, Ueber einige Begriffe und Aufgaben der Psychophysik, *Arch. f. d. ges. Psychol.*, 1913, 30, 116-118.

[21] Boring, *Psychol. Rev.*, 27, 449.

Under the *Einstellung* laid down by the instructional determinant for unitariness, *O* would have a wide and unchecked range for equivocal judgments which would issue in rank order of degrees of unitariness of which the scientific significance would be small.

CHART 1

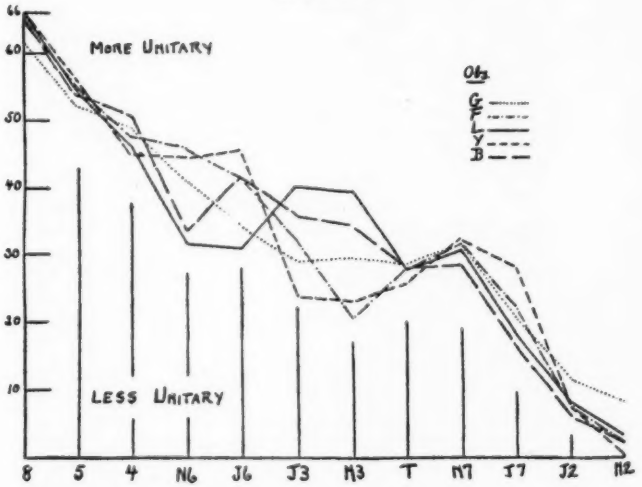

CHART 1. Rank-order of tonal intervals under instructions for 'unitariness' (see text). Method of paired comparisons. Ordinates represent number of discriminations for every interval for five *O*'s. Abbreviations: 8 = octave, 5 = fifth, 4 = fourth, N6 = minor sixth, J6 = major sixth, J3 = major third, N3 = minor third, T = tritone, N7 = minor seventh, J7 = major seventh, J2 = major second, N2 = minor second.

In Chart 1 are shown the curves derived from judgments of unitariness. In all the charts the intervals on the abscissa have been arranged in the same order for ease of intercomparison. The numbers on the ordinate represent the frequency of preferences. The curves of Chart 1 make clear the rather significant fact that *O*'s can take up a stimulus-attitude toward mental experience and come out with a surprising degree of uniformity. The net returns to psychology of such agreement of results are small, however, inasmuch as no light is thrown upon the mental processes involved in the judgments.

At the end of this series of experiments, the *O*'s were asked for introspective characterizations in which should be noted the criteria which had entered into or cut across the judgments of unitariness. These reports, together with the qualitative data secured from the earlier preliminaries, furnished the material out of which grew the experiments now to be considered.

Principal Series

The method and procedure in the following series were the same as in the foregoing experiment. The only difference lay in the instructions given to *O*. From the introspective data at hand were selected those factors in the *A, B, C, . . .* system of tonal mental processes which approximated the phenomenological level of experience. It was hoped by means of instructions and self-checking on the part of *O* to limit the judgments to a single factor, and thus come out with rank orders of which the significance would be more certain. Certain of the factors employed as criteria of judgment were common to the introspective reports of all the *O*'s; others, especially those of a contextual nature, followed the idiosyncrasies of individual differences and could be used as instructional determinants only for particular *O*'s. Before beginning work on a new criterion, one observational session was devoted to practice judgments, in order that *O* might familiarize himself with the new factor and become sufficiently *eingestellt* to take up a similar attitude in subsequent sessions. Whenever, during the series, *O* noted a lapse in attitude or found it impossible to make a judgment in accordance with instructions, he was expected to report the fact to *E*. At the end of every series *O* was requested to give a descriptive characterization of his attitude and basis of judgment.

Auditory Processes

Smoothness-roughness.—The first criterion to be used had as its sensory basis the 'smoothness' and 'roughness' of the auditory impressions. This factor had been frequently reported in all the introspections, and was easily taken up and followed through by all the *O*'s. The instructions for this series read as follows:

> "Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of 'smoothness' and 'roughness,' regardless of other possible characteristics of the impressions. Report 'first' or 'second' according as the first or second impression is 'smoother' or less 'rough.' If there is no difference between them, report 'same.' Whenever you fail to live up to instructions or find it impossible to make a judgment on the basis of the above criterion, report 'failure.'"

The purpose of such an instruction was to get *O* univocally determined in the direction of auditory 'smoothness-roughness' and away from other possible factors in the tonal impression.

The descriptive characterizations point to 'roughness' as the positive category in this series, and to 'smoothness' as the absence of 'roughness.' 'Roughness' is a temporal-intensive pattern in which intensity varies in time, whereas complexes in which intensity seems relatively constant are 'smooth.' Comparison-judgments between complexes in which the variation of intensity is rapid and those in which intensity variations are slow (minor thirds and major seconds *vs.* minor seconds) embarrassed some of the *O*'s. This difficulty they usually resolved by judging as 'smoother' those complexes in which the intensity variations are more rapid.

CHART 2

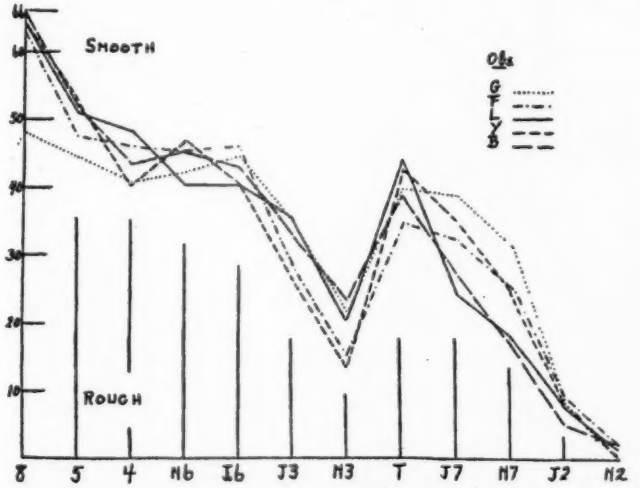

CHART 2. Rank-order of tonal intervals under instructions for 'smoothness-roughness' (see text). Curves for five *O*'s.

Chart 2 indicates the rather striking uniformity of rank orders obtained under this attitude. It will be noted that 'smoothness' and 'roughness' vary with the physical distance of the components of the interval: the octave, fifth, fourth, sixths, tritone, minor seventh, and major third all come higher

on the scale than the minor third and the seconds. The relatively low position of the major seventh must be due to the presence of overtones which cut across the factor of distance.

Simplicity and Complexity.—The next two criteria of judgment to be adopted took their root in the general phenomenon of analysability. The possibility of taking up two attitudes in this respect made it advisable to follow through separate series. The instructions in the first series, as, in point of fact, for all subsequent series, were worded like the instructions for 'smoothness-roughness,' except in those phrases intended to determine *O* in a new direction:

"Following the 'ready' signal, you will be presented with a successive pair of tonal complexes. In each case you are to compare the two impressions with respect only to degrees of 'simplicity.' By 'simplicity' is meant the degree of approximation to singleness of auditory core, *i. e.*, unanalysability of the impressions. Report 'first' or 'second' . . . " etc.

The instructions for the second series read:

". . . In each case you are to compare the two impressions with respect only to degrees of 'complexity,' *i. e.*, you are to state in which of the two impressions the components separate most during presentation. . . ."[22]

The introspective data for the 'simplicity' series indicate that the criterion here involves difficulty of analysis or independent variability of clearness of components in time, which occurs under a fairly active attitude (=attentional attempt at analysis). This is to say that those complexes in which it takes longer for the total impression to split up into its separate components, or in which one component dominates the other in clearness, are judged as more 'simple' inasmuch as they approximate more nearly to singleness of auditory core.

In the 'complexity' series, on the other hand, the judgments are made under a more passive attitude. *O* reported merely upon the relative "analysedness" of the impressions as given during presentation of the stimulus, and not upon 'analysability.'

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[22] During the practice period for this series it was discovered that the *O*'s were often uncertain in their judgments because of the change which the impressional contents for judging underwent during the formulation of the judgments in the course of the immediate memory after-images. Hence they were instructed to base their judgments upon the contents as they appeared *during presentation.* To facilitate this mode of judgment, the exposure time of each interval was reduced from 2. to 0.5 sec. (measured by a silent metronome).

In Chart 3 are shown the curves for the 'simplicity' series. The 'simplicity' curves for G and B turned out to be nearly identical with their 'complexity' curves, and have therefore been placed as G(s) and B(s) on Chart 4 ('complexity') instead of on Chart 3 (the other 'simplicity' curves). The three curves of Chart 3 follow fairly closely the Stumpfian order of degrees of fusion, except for the rather high positions of the tritone and minor seventh, a possible explanation of which will be mentioned presently. The 'complexity' curves deviate from the 'simplicity' curves in a uniform and significant manner. Those intervals of which the components lie far apart on the tonal scale (sixths and sevenths) occupy the lower positions on the chart, whereas those intervals of which the components are proximate (thirds and seconds) occupy uniformly higher positions. The high positions of the octave, fifth, and fourth, are undoubtedly referable to the factor of 'fusion'[23] which cuts across the factor of sense-distance. The opposed positions of the seconds on the two charts demand a word of explanation, an explanation forthcoming in the introspections, which show quite definitely that two different attitudes are operative in the case of the seconds: a 'meaning' attitude and a descriptive attitude. The *O*'s whose curves for the 'simplicity' series are represented in Chart 3 judged certain of the intervals 'less simple' on the basis of contents which carried the meaning of non-simplicity rather than on the basis of relative degrees of simplicity of the auditory experience *per se*. The following quotations from their introspective characterizations illustrate the point:

Ob. F. "In some cases one of the complexes comes as very rough. It has the meaning of being readily analyzable at once. How this meaning is present structurally I cannot say, although the basis of it seems to be roughness. There is a verbal characterization of it as 'less simple,' but there does not seem to be any auditory attentional analysis."

Ob. L. "I tend naturally to judge as not simple a complex which produces roughness, or one where the 'noise' stands out prominently."

Ob. Y. "In such a case [comparison of a rough complex with a smooth one, the components of which stand out clearly] the rough one is usually cognized as a complex of two tones, more immediately than the other. Yet, since it was very difficult to split up the rough interval into two components actually, *i. e.*, psychologically, I judge it as simpler."

[23] The word "fusion," when used in connection with the curves under consideration, bears no theoretical implications: it is merely a convenient term to apply to those intervals whose components either will separate out only with difficulty (=unanalysability) or do not fall apart during presentation (=unanalysedness).

Y's report shows that he was aware of his two attitudes, but his curve points to only a partial approximation to the psychological descriptive attitude: more often he judged the rough intervals on the level of cognizance and meaning. In the complexity series, however, he was able to abide quite rigidly by the descriptive attitude, as his curve indicates. L, too, was able to keep to the descriptive attitude fairly consistently in the 'complexity' series, although not to the extent

CHART 3

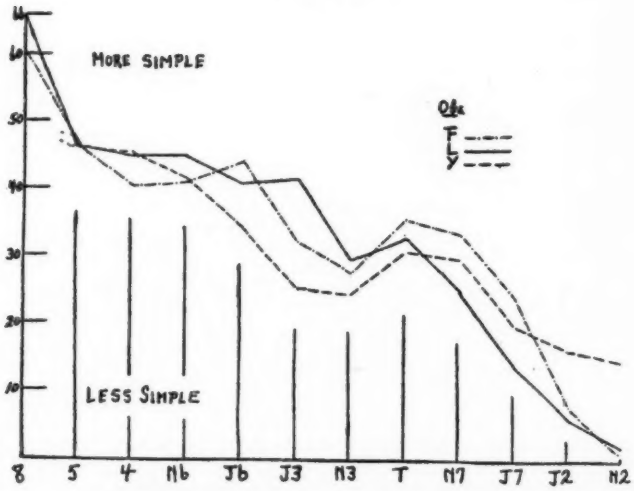

CHART 3. Rank-order of tonal intervals under instructions for 'simplicity' (see text). Curves for three *O*'s. 'Simplicity' curves for two *O*'s are shown in Chart 4.

of the other *O*'s; at least the low position of her curve at the seconds would seem to warrant such a statement. It is matter for regret that circumstances made it impossible for F to complete the 'complexity' series.

Regarding the high positions of the tritone and minor seventh in the 'simplicity' series, adverted to above, it may be that this very factor of attitude is operative. In the 'smooth-rough' series the tritone and minor seventh occupy relatively high places, and since 'roughness' was part-mediator for the meaning of 'less simple,' it is reasonable to sup-

pose that 'smoothness' may have entered in at times to support the meaning of simplicity, and thus give to the tritone and minor seventh values higher than would otherwise have been the case. It is not clear, however, why for two of the *O*'s the 'simplicity' series gave curves like the 'complexity' curves. A valid explanation can be sought only in repeated experiments under more stringent control of instructional determinants.

CHART 4

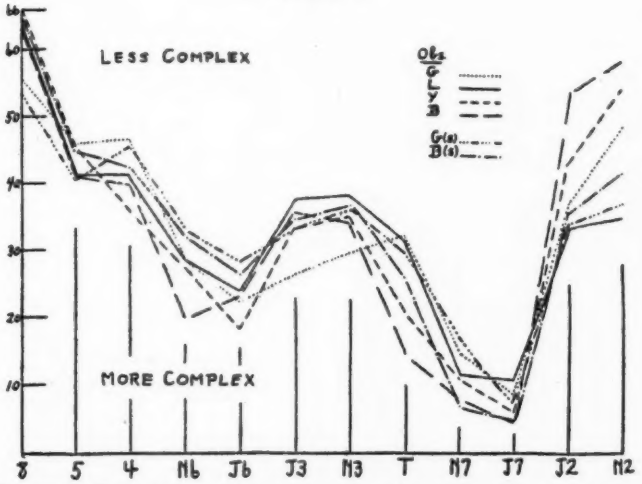

CHART 4. Rank-order of tonal intervals under instructions for 'complexity' (see text). The curves for G(s) and B(s) are 'simplicity' curves (see Chart 3). The four 'complexity' curves are inverted.

Pleasantness and Unpleasantness.—Although in the preliminary experiments, the *O*'s had stated that pleasantness and unpleasantness entered but seldom as disturbing criteria of judgment, it was deemed advisable to run through a series of *P* and *U* in order to discover just how judgments would come out under an affective determination. The instructions were similar to previous ones with the exception that *O* was requested to judge in terms of *P* and *U* only.

Under the conditions of the present experiment, *P* and *U*

turn out to be meanings for all *O*'s,[24] or at least experiences not *sui generis*. For F and Y, the introspective reports state the potency of 'smoothness' and 'roughness' as mediating factors in the affective judgment, a fact borne out by the resemblance of the curves of F and Y (Chart 5) to the 'smooth-rough' curves. The agreement of these two curves and the agreement of the other three curves, and the marked difference between the two types, are worthy of especial note.

CHART 5

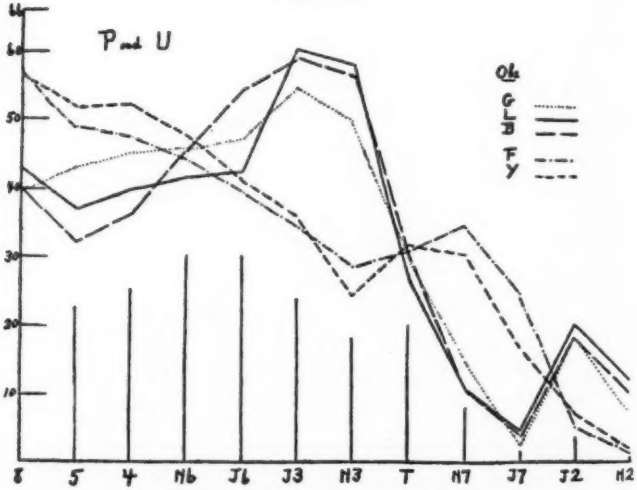

CHART 5. Rank-order of tonal intervals under instructions for *P* and *U*. Curves for five *O*'s.

For G, L, and B, whose curves represent the second type, the *P* and *U* judgments are explicitly reported as being on the level of meaning. Just what the sensory mediators for the judgments represented by these curves may be can not be satisfactorily gleaned from the introspective reports, although hints are not wanting to point to non-auditory concomitant processes of the auditory experience as playing a part. (See below: the "horrisonorous" curve for G, the "rotundity"

[24] For a recent exposition of the resemblance between affection and meaning, see M. Yokoyama, The nature of the affective judgment in the method of paired comparisons, *Amer. J. Psychol.*, 1921, 32, 357ff.

and "hard-soft" curves for L, and the "kinaesthesis" curve for B.) Perhaps the most interesting feature of these curves is their consistency with musical usage: the sixth and thirds are very pleasant; the minor seventh and seconds are unpleasant and the major seventh very unpleasant; the octave, fifth and fourth are rather "indifferent."

So far the judgments under varying attitudes have come from instructions the purport of which was to determine *O*

CHART 6

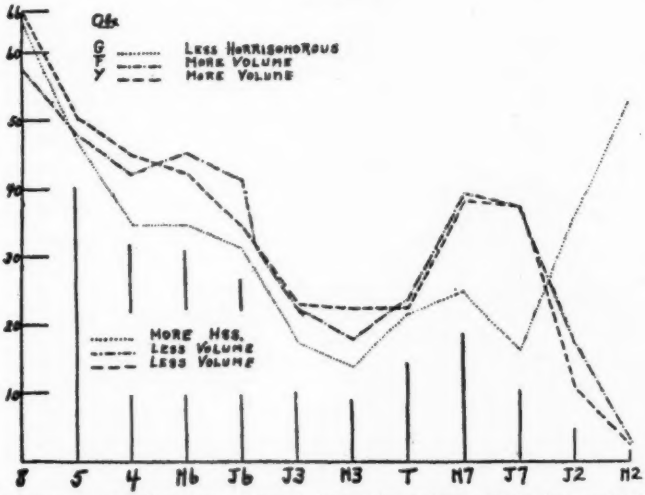

CHART 6. Rank-order of tonal intervals under criteria into which entered integrations of auditory and non-auditory processes (see text). Curves for three *O*'s. The curve for G is inverted.

in the direction of purely auditory aspects of the total contents set up by the stimulus-situation. In the qualitative analyses of the preliminary experiments, however, there were numerous references to non-auditory concomitant processes, or to integrations of auditory and non-auditory processes, which presented themselves as important parts of the whole impression or as strong candidates for judgment in the case of unitariness. For the sake of greater completeness, therefore, although an exhaustive study of all possible factors in the *A, B, C;*

. . . system was of course out of the question in the time at our disposal, it was decided to follow through certain of the more insistent contextual processes.

Integration of Auditory and Non-auditory Processes

The curves of Chart 6 represent rank orders from judgments based on an intimate integration of auditory and non-auditory processes. The criterion of this sort followed through by F was integration of the voluminous aspect of the auditory impression and a concomitant chest-kinaesthesis of varying degrees of expansiveness; a similiar criterion by Y, an integration of the same voluminous factor and a concomitant visual process between the two extremes of which a gap of varying width could be observed. In addition to the close uniformity of the two curves there should be noted also the factor of distance of components on the tonal scale, which is again operative: the low position of the thirds and seconds in which the paired components are close together, and the high position of the sevenths and sixths in which the paired components are farther removed from each other. Otherwise the form of the curves is similar to that for the 'simplicity' series. The criterion adopted by G in this group was an integration of auditory and kinaesthetic processes to which, for the sake of convenient reference, he originally gave the term "horrisonorous." By this term he meant the vibrant, rather unpleasant, factor in the auditory experience (*horridus*) together with the auditory fulness and richness of the impression. Although on closer observation G became convinced that the pattern of "horrisonorousness" consisted of an intimate integration of ear-kinaesthesis and auditory quality which could only be separated under careful attentive analysis, he still retained the term. The form of G's curve is similar to those for F and Y, and what has been said concerning these latter is equally true of the former. The one exception, in the case of the seconds, is explained in G's introspections by the fact that with the coming in of marked roughness the "horrisonorous" factor is 'swamped.' (The curve has been inverted on the chart.) Were it not for this rapid rise of the curve at the seconds, the resemblance to the other two curves would be still greater, inasmuch as a lowering of the curve at one point would result in an elevation at another, the essential event in this case for bringing about a greater agreement.

Concomitant Non-auditory Processes

Chart 7 gives the curves for judgments based on purely non-auditory concomitant processes. L, who is of a marked visual type, was able to make judgments on various aspects of what she called a 'pseudo-visual' image, *i. e.*, kinaesthesis of eye-movement which served as an effective surrogate for the actual visual experience. By 'rotundity' she meant the fulness and size of the visual image; by 'hard-soft,' the texture

CHART 7

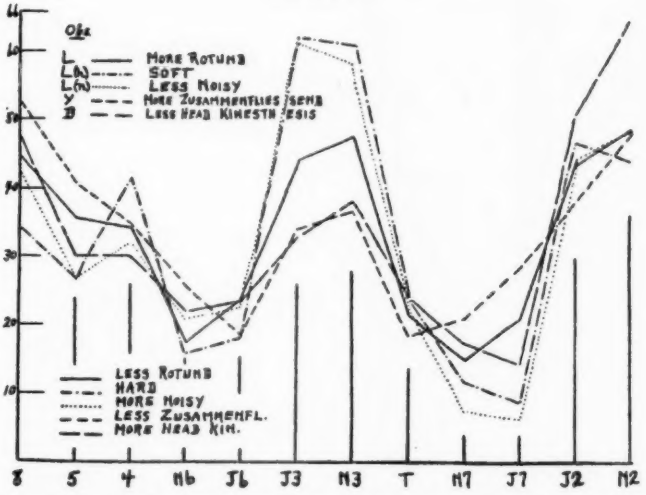

CHART 7. Rank-order of tonal intervals under criteria into which entered concomitant visual and kinaesthetic processes (see text). Curves for three *O*'s. The curves for L(h), L(n), and B are inverted.

of the image; and by 'noise component,' the jagged or even contour of the image. Y followed through a criterion which he called visual *Zusammenfliessen*, by which he meant the pulling together or apart of concomitant visual images in the field of vision. By 'head-kinaesthesis' B referred to varying degrees of extension of concomitant kinaesthesis across the posterior portion of the head. It can be seen at a glance that there is less agreement among the curves of Chart 7; nevertheless a marked directional uniformity is apparent at the

TABLE I

FREQUENCY OF JUDGMENTS UPON TONAL INTERVALS UNDER VARIOUS CRITERIA. METHOD OF PAIRED COMPARISONS. COLUMN AT LEFT REFERS TO PARTICULAR CRITERION OF JUDGMENT. SEE CHARTS AND TEXT. ABBREVIATIONS: 8 = OCTAVE, 5 = FIFTH, 4 = FOURTH, J = MAJOR, N = MINOR, T = TRITONE

Criterion	Obs.	8	5	4	N6	J6	J3	N3	T	N7	J7	J2	N2
Chart 1 Unitariness	G	61½	52	49	41	34	29	29½	28½	31½	20½	11½	8
	F	64½	55	47½	45½	41½	32	20½	27½	31½	22	7½	1
	L	66	54½	46	31½	31	40	39½	27½	30½	18½	8	3
	Y	66	55½	45	44½	45½	23½	23	25½	32	28	7½	0
	B	65½	54	50½	33½	41½	35½	34½	28	28½	16½	6	2
Chart 2 Smooth-Rough	G	48	44½	41	42½	44½	35	21½	40	39	31½	8½	0
	F	63	47½	46	45½	46	28½	15½	35	32½	25½	9	2
	L	64	51	48½	40½	40½	35	21	44½	24½	18	7½	1
	Y	64	52½	40½	47	40½	26½	13½	43	35	25	8½	0
	B	65½	51½	43½	45½	43	33	23½	39	27	17½	5	2
Chart 3 Simplicity	F	60½	46½	40½	41	44	32½	27½	35½	33½	24½	8½	1½
	L	66	46½	45	45	41	41½	29½	33	25	14	7	2½
	Y	66	46	45½	41½	34½	25½	24½	31	30	20	16½	15
Chart 4 Complexity	G (s)	53½	40½	45½	33½	28½	33½	36	29½	17	7½	34	37
	B (s)	65	45	42	32	26½	34½	36½	25½	7	5	35½	41½
	G	55½	46	46½	28½	22½	26½	29½	32	15	9	37	48
	L	64	41	41	28½	24	37½	38	31½	11½	11	33½	34½
	Y	66	45½	35½	27½	18½	33½	35	20½	11	6	43	54
	B	63½	41	40	20	23	35½	34½	14½	8	4½	53½	58

Chart 5 *P* and *U*	F	57	49	47½	44	39½	34½	28½	30½	34½	24½	5	1½
	Y	56½	51½	52	47½	41	35½	24½	31½	30½	16½	7	2
	G	39½	43	45	45½	47	54½	49½	29½	15	2½	18	7
	L	43	37	39½	41½	42½	60	58	27	11	4½	20	12
	B	40	32	36	45½	54	59	56½	30½	11	3½	18	10
Chart 6 Auditory and Non-auditory Integrations	G	64½	47	35	35	31½	17½	14	21½	25	16½	36	52½
	F	57½	48	42½	45	41½	22½	18	23½	39½	37½	17½	3
	Y	66	50½	45	42½	34½	23	22½	22½	38½	37½	11	2½
Chart 7 Non-auditory Processes	L	44½	35½	34	17½	23½	44	47½	21½	15	21	43½	48½
	L (h)	34	27	41½	16	18	62	61	24	12	9	47½	44
	L (n)	43	27	32	21	23½	61½	58	23	7½	6½	44½	48½
	Y	52½	40½	34½	25½	18½	34	36½	18½	21	28½	38	48
	B	47½	30	30	22	23	33½	38	24	17½	14½	51	65

position of certain of the intervals. As in Chart 6 the 'distance' factor is operative here, although in the opposite direction: the thirds and seconds (intervals whose components are proximate) rank relatively high, and the sevenths and sixths (distant components) low. Unusually low positions of the octave, fifth, and fourth are undoubtedly due to the violence with which the 'distance' factor intrudes upon the 'fusion' factor: note the extremes attained by the thirds and sevenths.

Conclusions

The numerical data from which the charts have been drawn are summarized in Table 1. The fractional numbers there derive from the usual procedure in the method of paired comparisons of giving equal value (*i. e.*, ½) to each member of a pair in an equality judgment.

The results indicate clearly the complexity of the tonal impression that is dependent upon the presentation of bitonal stimuli. Various factors which make for this complexity may be isolated by means of carefully controlled instructional determinants and examined apart. Such isolation and examination as the present study has undertaken bring to view certain of the phenomenological characteristics which constitute the foundation of bitonal impressions.

Smoothness-roughness.—The continuum to which the expression 'smoothness-roughness' applies is best described in terms of 'roughness.' By 'roughness' the *O*'s meant a temporal-intensive pattern in which various rates of intensive oscillations of the auditory experience are observable. 'Roughness' is inversely proportional to the rapidity of intensive oscillations. 'Smoothness' is the absence, or relative absence, of intensive oscillations. Absolute 'smoothness' is a continuation in time of the auditory experience without observable variations in intensity.

Simplicity.—By 'simplicity' the *O*'s intended the phenomenon of unanalysability. The introspective reports show that by unanalysability they meant the covariation in sensory clearness of the auditory components under an active attitude (=attentional analysis) by virtue of which isolation of one component becomes difficult, that is to say, occurs only incompletely or only after a considerable interval, or else, on occurring, persists only for a short time. The tonal complexes are judged increasingly less 'simple' as the variation in clearness of the components becomes more independent.

Complexity.—For judgments of 'complexity' the *O*'s assumed a more passive attitude, *i. e.*, an attitude for judging

upon the 'complexity' of the interval as it appeared *during presentation*, rather than an active attempt at attentional analysis. From the introspective characterizations it becomes apparent that the *O*'s understood by 'complexity' the degree of disparity (nearness or remoteness on a scale of 'sense-distance') of the auditory pitch qualities as they appear in the tonal impression. Those intervals in which the components are readily perceived as disparate with respect to pitch quality are judged as 'complex,' whereas those intervals in which the pitch qualities are less readily perceived as separated or dual are judged as less 'complex.'

From a consideration of Chart 4 it becomes apparent that 'complexity' varies with physical distance: those complexes in which the components lie further apart on the tonal scale are judged more 'complex' than those complexes in which the components are proximate. Exceptions occur, however, in the case of the fourth, fifth, and octave. It would seem that the qualitative disparity of these intervals is obscured by a greater covariation in clearness of the components, the factor which makes for 'simplicity.' That is to say, processes which ordinarily lead to the judgment 'complex,' are cut across in the case of the octave, fifth, and fourth by more insistent processes leading to the judgment less 'complex'=more 'simple.' It should be noted that in the characterizations of 'simplicity' all the *O*'s except one had occasion to speak of those intervals in which covariation in clearness made analysis difficult as "stuck together," "glued together," "well knit," "well fused," "hanging together," *etc.* Such characterizations of the impressions, together with the fact that covariation in clearness cuts across and obscures qualitative disparity, make plausible the statement that covariation in clearness is related to a true perceptual 'fusion' of tones, *i. e.*, fusion is an integration of attributes of clearness rather than a qualitative integration.

Pleasantness and Unpleasantness.—None of the *O*'s based their affective judgments upon *P* and *U per se*. In all cases the judgments were of the nature of attitudinal or 'meaning' responses. For two *O*'s *P* and *U* turn out to have their sensory bases in 'smoothness' and 'roughness.' For convenience of reference the affective preferences of these two *O*'s may be designated as judgments of 'smooth' *P* and *U*. The other three *O*'s, whose affective preferences accord more with musical usage, were unable to give any thoroughgoing characterization of the sensory mediators for *P* and *U*. In lieu of a better term their affective judgments may be referred to as 'musical' *P* and *U*.

Volume.—Two *O*'s found it possible to characterize the intervals in terms of 'voluminousness' or 'fulness.' It is evident from their descriptive reports that 'volume' for them is not a purely auditory affair, but an integration of auditory process with certain contexts which attach to the intervals. For one *O* 'volume' is an integration of auditory process and chest-kinaesthesis; for the other, an integration of auditory and visual processes.

Horrisonorousness.—By this term one *O* meant an intimate integration of auditory quality and ear-kinaesthesis into a closely knit pattern of vibrant qualitative roughness. Originally, in the preliminary series, this *O* noted that 'horrisonorousness' was frequently affectively toned. The agreement of this *O*'s affective preferences with certain of his judgments on 'horrisonorousness' makes it possible that the latter and the sensory basis of 'musical' *P* and *U* have something in common.

Visual and Kinaesthetic Processes.—Three *O*'s followed through series in which judgments were made upon concomitant non-auditory processes alone. Terms such as 'rotund,' 'soft,' and 'noisy' refer to the size, smooth contour, and jagged texture of concomitant visual processes; 'zusammenfliessend' to the pulling together or apart of visual imagery; and 'head-kinaesthesis' to the spread of kinaesthetic process in the posterior portion of the head. (See Chart 7.)

It is now possible, with the aid of the foregoing terms, which stand for definite sensory experiences ('musical' *P* and *U* excepted), to characterize every interval separately with respect to various factors in the total impression which contribute to its constitution.

Octave.—The octave occupies an unique position among the tonal intervals in that it lies at one extreme of three of the attributive continua within which all intervals vary. It is the 'smoothest,' 'simplest,' and least 'complex' interval, in the sense of the discussion of the preceding paragraphs. With respect to 'smooth' affective judgments the octave is also the most pleasant interval. *O*'s whose affective preferences are of the 'musical' kind, however, find the octave about as pleasant as the fourth and fifth and less pleasant than the sixths and thirds.

By some *O*'s the octave was also characterized as giving a more 'voluminous' impression than the other intervals.

Fifth.—This interval resembles the octave in that it is marked by an almost complete absence of 'roughness.' One *O*, in fact, judged the fifth as practically equal to the octave

in 'smoothness.' The components of the fifth admit of more independent variation in clearness than do the components of the octave, that is to say, the fifth is slightly less 'simple.' This factor of 'simplicity' tends to predominate over the wide 'sense-distance' included in the fifth, so that the interval appears as less 'complex' than any of the other intervals except the octave. Under a 'musical' affective determination the fifth is slightly less pleasant than the octave except for one *O* who finds it somewhat more pleasant than the octave. The fifth is also less pleasant than the octave for those *O*'s whose affective judgments are mediated by 'smoothness.'

The contextual aspect of 'volume' when reported was more prominent in the fifth than in any other interval except the octave.

Fourth.—With respect to 'smoothness' the fourth is on an equal plane with the sixths, which are a little less 'smooth' than the fifth. In this interval the 'simplicity' factor again appears to obscure the 'complexity' aspect, so that only the fifth and octave are judged less 'complex' than the fourth. The fourth, fifth, and minor sixth all appear about equally 'simple.' 'Musical' affective judgments place the fourth before the fifth and the octave in *P*; 'smooth' affective preferences place the octave and fifth before the fourth in *P*.

For G, who judged upon a qualitative pattern of integrated auditory process and ear-kinaesthesis, the fourth assumed a trace of 'horrisonorousness.' The 'voluminous' characteristic, when it was observed, entered into the fourth to the same extent as in the sixths, which are exceeded in 'volume' only by the fifth and octave.

Minor Sixth.—The limits which various characteristics may reach under attitudinal changes vary more between intermediacy and extremes in the minor sixth than in the three preceding intervals. The fourth and both the sixths are very 'smooth' intervals and are exceeded only by the fifth and octave in this respect. The remoteness of the pitch qualities in the minor sixth lends to the interval an aspect of 'complexity' which is more marked only in the major sixth and the sevenths. On the other hand, under an active attitude of attentional analysis, the relative lack of independent variability in clearness of the components leads to the judgment 'simple' in the case of three *O*'s, that is to say, perceptual 'fusion' is more insistent than qualitative disparity. 'Musically' the minor sixth is more pleasant than any of the

preceding intervals. When mediated by 'smoothness,' however, it is less pleasant than the preceding intervals.

For two *O*'s, only the octave and fifth exceeded the minor sixth with respect to 'voluminousness.' G found 'horrisonorousness' present in this interval to the same extent as in the fourth. For certain *O*'s contextual and kinaesthetic processes became palpable in the minor sixth to an extent which would tend, under an equivocal determination, to obscure the purely auditory aspects of the impression.

Major Sixth.—The qualitative disparity of the components of the major sixth is even more marked than in the minor sixth, giving thus to the interval an aspect of 'complexity' exceeded only by the sevenths. Under the determination for 'simplicity,' however, three *O*'s find but little difference in 'simplicity' between the major sixth and the minor sixth and fourth. The major sixth is on equal terms with the minor sixth and fourth with respect to 'smoothness.' Only the thirds are pleasanter than the major sixth under a 'musical' affective determination. The other affective judgments place the major sixth just below the preceding intervals in *P*.

'Volume' was somewhat less and 'horrisonorousness' slightly more apparent in the major sixth than in the preceding intervals, for the *O*'s who found these contexts. As in the minor sixth, kinaesthetic and visual processes may have come in to obscure the auditory processes in the major sixth.

Major Third.—The relative position with respect to the other intervals of the major third, and also the minor third, varies considerably under attitudinal changes. Between the major third and the intervals already mentioned there is a marked difference in 'smoothness.' Only the minor third and the seconds are 'rougher' than the major third. Only the octave, fifth, fourth, and seconds are less 'complex,' and the octave, fifth, fourth, and sixth more 'simple,' than the major third. The relative 'simplicity' and 'non-complexity' of this interval are presumably not due so much to the covariation of clearness and the parity of pitch qualities of the components as to the obliteration of these processes by the rapid intensive variations of the temporal 'roughness' pattern. The major third is the most pleasant of all the intervals for those *O*'s whose affective judgments have a 'musical' mediation, whereas it is "indifferently" toned for the two other *O*'s.

The auditory-kinaesthetic context of 'horrisonorousness' was very prominent in this interval for G. For F and Y the 'voluminous' context of the major third was almost lacking. The auditory factors of the major third may also have been

considerably obscured by visual and kinaesthetic concomitant processes.

Minor Third.—With the exception of the preceding interval, the minor third is the most pleasant interval for those *O*'s whose affective judgments are of the 'musical' sort. The interval is rather unpleasant for those *O*'s whose affective preferences are mediated by 'smoothness.' One of the most striking characteristics of this interval is the prominence of the factor of 'roughness.' Only the seconds, and for two *O*'s the major seventh, are 'rougher' than the minor third. As with the major third, the temporal-intensive pattern of 'roughness' seems to blot out the separate auditory qualities with the result that the minor third is judged as relatively 'non-complex.' Three *O*'s find the minor third, however, less 'simple' than any of the preceding intervals because of a shift to a 'meaning' attitude in the presence of the 'rough' intervals; that is to say, intervals in which oscillations of intensity are very prominent are cognized as 'non-simple' by virtue of this very factor of 'roughness.'

'Horrisonorousness' was more prominent in the minor third than in any other interval for G. Concomitant non-auditory processes manifested themselves to a great extent in the minor third.

Tritone.—In none of the aspects which enter into tonal impressions does the tritone seem to occupy an extreme position with respect to the other intervals. Perhaps the most striking characteristic of the interval is its 'smoothness.' Only the group of intervals comprising the octave, fifth, fourth, and sixths, is smoother than the tritone. The qualitative disparity of its components is sufficiently marked to cause the interval to be judged as more 'complex' than the octave, fifth, fourth, thirds, and seconds, though not so 'complex' as the sixths and sevenths. For three of the *O*'s the tritone appears somewhat more 'simple' than the thirds. For all the *O*'s the tritone is "indifferently" affective.

G found 'horrisonorousness' rather well marked in the tritone, although not to so great an extent as in the thirds.

Minor Seventh.—The 'roughness' of the minor seventh is not very obvious, although, of course, the interval is not so 'smooth' as the tritone and the group of intervals already mentioned. It is about equal in 'roughness' to the major third. The qualitative disparity in pitch of the components of the minor seventh lends to the interval an aspect of 'complexity' only exceeded by the major seventh. Three *O*'s under a determination for analysability, however, find the minor

seventh rather 'simple,' *i. e.*, the independent variability in clearness of its components is not very marked. The interval is unpleasant for *O*'s whose affective judgments are mediated by 'smoothness' as well as for those whose judgments are 'musically' mediated, although less pleasant for the latter than the former.

O's, who judged on the basis of 'volume,' found the minor seventh very 'voluminous.' Other contextual processes were also apparent to a noticeable extent for some *O*'s.

Major Seventh.—With the exception of the seconds, and for three *O*'s the minor third, the major seventh is the 'roughest' interval. It is also the most 'complex' interval. Only the seconds, when cognized as 'non-simple' under a 'meaning' attitude, are judged less 'simple' than the major seventh. The interval is very unpleasant for all *O*'s. For the 'musical' type it is the most unpleasant, while for the 'smooth' type the seconds are somewhat more unpleasant. It can be seen that the extreme position which the major seventh occupies within certain continua tends to give it a unique status antipodal to the octave.

Only the thirds exhibited greater 'horrisonorousness' than the major seventh. For some *O*'s visual and kinaesthetic processes and the contextual 'voluminous' aspect entered very prominently into this interval.

Major Second.—Unquestionably the most striking feature of the major second lies in the violent intensive oscillations constituting the 'roughness' pattern. Only the minor second possesses this characteristic in a more pronounced degree. For all *O*'s the major second is very unpleasant. Under a 'meaning' attitude the seconds are cognized as the least 'simple' intervals; but under a strictly descriptive attitude they scarcely submit to psychological separation at all, *i. e.*, the auditory qualities can not be singled out in the face of the violent intensive variations which dominate the seconds to so great an extent.

The contextual factor of 'volume' dropped out in the seconds, as did also 'horrisonorousness.' The interval was rich, however, in kinaesthetic and visual processes.

Minor Second.—This interval is the 'roughest' of all. Under an affective determination of the 'smooth' type it is the most unpleasant interval. With respect to other aspects the minor second is similar to the major second: under a 'meaning' attitude an *O* knows that a violently 'rough' interval must be 'non-simple;' but under a descriptive attitude he

judges it as 'non-complex,' since the component qualities can not be separated out.

From the charts and the immediately preceding characterizations of the intervals it can be seen that the rank-order of certain bitonal complexes is more affected by change of attitude than is the rank-order of others. The thirds and sixths, *e. g.*, undergo greater alteration in position under varying instructional determinants than do the octave, fifth, and fourth. The seconds shift position very decidedly in the presence of a descriptive attitude as against a 'meaning' attitude; but since these differences of attitude are of a different order from those brought about by the more readily effective instructional determinants, the variation of the seconds may be left out of account in the present consideration. In the literature upon tonal fusions there is considerable agreement regarding the ranks of the octave, fifth, fourth, and seconds;[25] but the sixths, thirds, and sometimes the minor seventh, are variously placed by different experiments. In the present experiment the results of the 'unitariness' series (Chart 1) show a like agreement among *O*'s of the octave, fifth, fourth, tritone, sevenths, and seconds, and a considerable divergence for the thirds and sixths. The preceding discussion has shown that the sixths and thirds vary in rank more than the other intervals under change of instructional determinant. The difference becomes apparent if the averages of all the intervals of all the series, except the 'unitariness' series, are taken and their M. V.'s computed, and the intervals arranged in order in terms of decreasing size of M. V. This order is as follows: minor third, major sixth, minor sixth, major third, minor seventh, major seventh, octave, fifth, tritone, fourth, and seconds. It seems to indicate that the possibility of equivocal determination of judgment is greater for the sixths and thirds, and occasionally for the sevenths, than for the other intervals. This fact may explain the lack of agreement in the literature concerning the rank-order of these particular intervals, since the instructional limitation of an *O* to a particular sensory criterion is extremely difficult at best, and, in the absence of a previous knowledge of the phenomenological criteria available in tonal intervals, almost impossible.

[25] See *e.g.*, R. Schulze, Ueber Klanganalyse, *Philos. Stud.*, 1898, 14, 483; Stumpf, *Tonpsychologie*, 1890, ii, 135; A. Meinong and St. Witasek, Zur experimentellen Bestimmung der Tonverschmelzungsgrade, *Zeits. f. Psychol.*, 1897, 15, 191, 193; A. Faist, *ibid.*, 109-113; Kemp, *op. cit.*, 186-190; Pear, *op. cit.*, 66.

ON ARTERIAL EXPANSION

By G. N. Hartman and D. L. McDonough, University of Pennsylvania

Such a great deal of experimentation has been published on plethysmographic work that some time ago Leschke[1] attempted to reach some conclusions by submitting the results of all these studies to a statistical treatment. All of this work, however, has been qualitative; the authors have sought to determine whether the volume of the blood at the periphery increased or decreased. We are unable to find any study in which the amount of increase or decrease has been quantitatively studied. Such a quantitative determination is the object of the present study.

A Lehmann plethysmograph was employed to the glass tube of which an arbitrary scale was attached. The volume of the body of the plethysmograph and of each division of the scale was determined. When the subject was placed in the instrument, the water was taken from a large graduated tube. The volume of the arm was thus determined by subtracting the volume of the water used from the total volume of the plethysmograph.

Three series of determinations were made on each of 24 adult subjects. (1) A Stanton sphygmomanometer was placed above the elbow of the arm used. The sac was then inflated until the diastolic pressure was obtained. In this case the veins were closed while the arteries remained open, and hence the blood backed up in the forearm expanding the arteries to the limits of their elasticity. After this determination the sphygmomanometer was removed. (2) The subject was then instructed to hold his breath for as long a time as possible and a second determination was made. (3) The third determination was made during mental work; mental multiplication of a three-digit by a two-digit number being sufficient, in most cases, to keep the subject concentrated for a period of several minutes. At the beginning of each determination, the reading of the height of the column of water in the tube was recorded, as was also the *greatest* extent of change during the experiment.

[1] Leschke, E., Die körperlichen Begleiterscheinungen seelischer Vorgänge, *Arch. f. d. ges. Psychol.*, 21, 1911, 435ff. Die Ergebnisse und die Fehlerquellen der bisherigen Untersuchungen über die körperlichen Begleiterscheinungen seelischer Vorgänge, *ibid.*, 31, 1914, 27ff.

Subject	Volume of Arm in C. C.	Volume Change Diastolic in C. C.	Volume of Change in C. C. Holding Breath	Volume of Change in C. C. Mental Work	% of Change Diastolic	% of Change Holding Breath	% of Change Mental Work
1	708	12.5	0.0	0.0	1.77	0.00	0.00
2	732	7.5	—1.2	—2.5	1.02	0.17	0.34
3	1103	17.5	0.0	0.0	1.59	0.00	0.00
4	840	5.0	—5.0	—1.2	0.60	0.60	0.15
5	845	15.0	—1.2	—1.2	1.77	0.15	0.15
6	808	10.0	0.0	—1.9	1.24	0.00	0.23
7	888	16.2	0.0	1.2	1.83	0.00	0.14
8	1111	8.8	—3.1	0.0	0.79	0.28	0.00
9	1400	17.5	—3.8	—2.5	1.25	0.27	0.18
10	773	5.0	—1.2	—1.2	0.65	0.16	0.16
11	830	21.2	—7.5	—1.2	2.60	0.91	0.15
12	810	11.2	—2.5	—1.2	1.40	0.31	0.16
13	985	45.0	—1.2	2.5	3.17	1.25	0.17
14	782	15.0	1.2	1.2	1.90	0.16	0.16
15	788	12.5	0.0	—1.2	1.60	0.00	0.16
16	908	22.5	—1.2	2.5	2.48	0.14	0.28
17	1022	17.5	—0.6	1.2	1.70	0.06	0.12
18	812	7.5	—0.6	—1.2	0.92	0.01	0.15
19	1045	16.2	—1.2	0.6	1.56	0.12	0.06
20	838	22.5	—2.5	—2.5	2.68	0.29	0.29
21	955	20.0	—2.5	—3.7	2.01	0.25	0.38
22	1030	37.5	10.0	—1.2	2.75	0.67	0.08
23	1078	30.0	—2.5	1.2	2.78	0.23	0.12
24	1142	25.0	2.5	3.7	2.19	0.22	0.03
Ave.	926	17.9	2.2	1.5	1.76	0.26	0.16

The results are found in the accompanying table. In the first column are indicated the subjects by number. In the next column the volume of the arm under normal conditions is recorded. In the next three columns are the volumes of change in c. c. respectively under the conditions of diastolic pressure, of holding the breath, and of mental work. A *minus*-sign indicates a decrease in volume. Inasmuch as the volume of the arm varies considerably for the different subjects, the actual amount of increase or decrease loses in significance when the different subjects are compared. Hence the percentage of change was calculated for the different experimental situations. These values are found in the last three columns of the table. The averages for the values in each column are found in the bottom row.

It will be noticed that the changes under the conditions of diastolic pressure are very much larger, on the average, than those for either mental work or holding the breath. The average for diastolic pressure is 1.76 *per cent.*, the greatest change being 3.17 *per cent.* (Subject 13), the smallest 0.60 *per cent.* (Subject 4). In the case of holding the breath, the average change is 0.26 *per cent.*, the greatest being 1.25 *per cent.* (Subject 13); while for Subjects 1, 3, 6, 7, and 15 no change was noted. For mental work, the average change was 0.16 *per cent.*, the greatest change being 0.38 *per cent.*; while for Subjects 1, 3 and 8 no change was noted. The change for diastolic pressure was greater than that for holding the breath in every case but one (Subject 4) where it was equal but in the negative direction. The change for diastolic pressure was invariably greater than for mental work. In 10 cases the change for holding the breath was greater than for mental work, in 6 cases it was equal, and in 8 cases less.

Summary

(1) This study suggests a method for dealing with volumetric peripheral vascular changes in a quantitative way.

(2) Under our conditions we were unable to get increases in volume of blood in the forearm which approached the limits of elasticity of the vascular system.

(3) The combination of the sphygmomanometer with the plethysmograph appears to give a better determination of arterial elasticity than does the present medical clinical method.

FUNCTIONAL PSYCHOLOGY AND THE PSYCHOLOGY OF ACT: I[1]

By E. B. TITCHENER

§ 1. In the dichotomy by Extension, physical phenomena receive positive and psychical phenomena receive negative determination. In another dichotomy, which is perhaps even more familiar, this relation is reversed: the universe of experience is divided into the Conscious and the Not-conscious, and psychical are identified with conscious phenomena, mind with the totality of consciousness. Psychology thus receives at length a positive definition.[2]

The word 'consciousness' is, however, notoriously ambiguous;[3] and the question whether the adjective 'conscious' suffices to mark off a special class of phenomena must therefore be discussed with great care. It must, moreover, be discussed in two separate contexts. For the psychologists who recognise the independent existence of 'phenomena of consciousness,' as the given objects of psychological investigation, fall at the present time into two principal groups. The one of these emphasises, in a psychology of 'function,' the biological aspect of empirical psychology; the other, in a psychology of 'act,' emphasises its intentional aspect.[4] Both alike may

[1] In writing a chapter on "The Definition of Psychology: Subject-Matter," for the introductory volume of a Systematic Psychology, I have been obliged to take critical account of functional psychology and the psychology of act. Since it seems fruitless to publish conclusions without giving the evidence on which they rest, and since at the same time a book such as I have planned is not the place for these criticisms of detail, I print my comments in the JOURNAL. The present article deals with functional psychology; a following article will deal with the psychology of act. The first section of the text follows a discussion of the definition of mind, negatively, by "absence of extension" (Bain).

[2] Moreover, the Not-conscious may be further subdivided, in the interest of biology, into a Living and a Not-living. There can be no doubt that, formally regarded, this classification is superior to that by Extension.

[3] The *locus classicus* is A. Bain, *The Emotions and the Will*, 1880, 539ff. Cf. also R. Eisler, *Wörterbuch der philosophischen Begriffe*, i., 1910, 177ff.

[4] Functional psychology, in this sense, is especially American, and the psychology of act especially German. Recent English psychology, through G. F. Stout (*Anal. Psychol.*, i., 1896, 36, 40), has been influenced by Brentano.

therefore trace their descent from Aristotle.[5] But in spite of much that they hold in common, the difference of motivation makes it necessary to consider the two groups separately.

In dealing with 'functional' psychology, we shall first examine a single representative system, and shall then bring together, for critical review, what appear to be the major tenets of the school. Systems that are based upon the concept of consciousness, even if this be taken under a single aspect, will inevitably differ; and we find accordingly that the biological emphasis falls differently in different functional psychologies. For Ladd, the 'stream of consciousness' is a life, and mind, the totality of consciousness, is an organism, "a unique and living totality in a course of development,"—an organism living a life of its own, which is always connected with the bodily life, but yet is "in some sort independent of" the body; and functional psychology is the science of the functions or activities of this psychical organism.[6] Angell, on the other hand, regards consciousness as an organic function,[7] a phenomenon of control;[8] "mind," he declares, "seems to involve the master devices" whereby the "adaptive operations of organic life may be made most perfect."[9] Judd, again, appears to combine both views. At first, consciousness is represented as an organic function, as something which, like the digestive or locomotive function, plays its part in the economy of the organic life under the conditions of the biological struggle for existence; it is thus one of the 'attributes' of man, considered as the highest animal.[10] Later, consciousness is 'possessed' by a knowing self, an unitary being which

[5] Intentionalism is ascribed to Aristotle on the ground of his doctrine of the relation of sensation to object of sense (*De anima*, 424*a*, 425*b*), and of thought to the object of thought (*ibid.*, 429*a*, 430*a*, 431*b*). Cf. also his statement that the objects of memory and imagination (*De mem.*, 450a) and those of thought and desire (*De an.*, 433*a*; *Met.*, 1072*a*) are identical.

[6] G. T. Ladd, *Psychology, Descriptive and Explanatory*, 1894, 638, 659ff.; *Philos. of Mind*, 1895, 400ff., esp. 405; G. T. Ladd and R. S. Woodworth, *Elements of Physiol. Psychol.*, 1911, 656ff.

[7] J. R. Angell, *Psychology*, 1904, 79. In 1908, 95 the phrase is dropped, but the meaning of the passage appears to remain the same.

[8] Angell, "The Province of Functional Psychology," *Psychol. Rev.*, xiv., 1907, 88.

[9] *Psychol.*, 1908, 8. In 1904, 7, "mind seems to be the master device." Cf. also 1904, 50, 86 with 1908, 64, 103. In both editions mind is "an engine for accomplishing the most remarkable adjustments of the organism to its life conditions:" 1904, 379; 1908, 436.

[10] C. H. Judd, *Psychology*, 1917, 4, 161; "Evolution and Consciousness," *Psychol. Rev.*, xvii., 1910, 84.

finds its nearest analogy in the living organism.[11] Differences of this sort find their natural expression in the classifications and arrangements and relative emphases of the writers' systematic works. But they have little if any bearing on the question immediately before us; they do not destroy the essential unity of the functional school.

The psychology of 'act' cannot be treated in the same comprehensive way. For here, as we quickly recognise, individual differences are not only inevitable but also fundamental and constitutive, so that the principal systems and programmes must be separately considered. Brentano, of course, furnishes both our starting-point and our constant point of reference. Besides Brentano, we pass under review, either in their own person or in that of some member of their school, Meinong, Stumpf, Lipps, Husserl and Külpe. We then interrupt the course of the exposition, in order to compare and contrast two experimental text-books of intentionalistic psychology, Witasek's *Grundlinien der Psychologie* (1908), which systematises Meinong's views, and Messer's *Psychologie* (1914), which we may regard as, in large measure, a systematisation of the later views of Külpe. The digression will be useful: it will reveal likeness and difference, in kind and in degree, and will thus prepare us for a broader survey of the act-systems. But here, we repeat, it proves impossible to follow the lines laid down by our study of functional psychology. We shall rather enquire, first, into the significance for these systems of the classification of psychical phenomena, and secondly into their treatment of two modes of such phenomena, namely, sensation and attention. The choice of topics is not arbitrary. It is suggested by the foregoing review of the principal systems; and it leads us to certain general conclusions with respect to intentionalism at large.

Ladd's Systematic Psychology

§ 2. We may take, as broadly typical of the functional systems, Ladd's definition of psychology: "the science which describes and explains the phenomena of consciousness, as such."[12] Consciousness, Ladd says, cannot in strictness be defined. Its meaning may, however, be brought out by contrast. "What we are when we are awake, as contrasted with what we are when we sink into a profound and perfectly dreamless sleep, or receive an overpowering blow upon the head—*that* it is to be conscious. What we are less and less

[11] *Psychology*, 274ff. Cf. below, p. 532.
[12] G. T. Ladd, *Psychology, Descriptive and Explanatory*, 1894, 1.

as we sink gradually down into dreamless sleep, or as we swoon slowly away: and what we are more and more, as the noise of the crowd outside tardily arouses us from our after-dinner nap, or as we come out of the midnight darkness of the typhoid-fever crisis—*that* it is to become conscious."[13] For the rest, he commits his case to the "reflective mind of all mankind." "The distinction between external facts and facts of consciousness, as actually made by every man, furnishes . . . the one peculiar and abiding standpoint of psychology."[14]

It would be foolish, certainly, to require a definition of the indefinable. If we are to deal with the indefinable, we can ask only that it be exhibited. We must insist, however, that it be exhibited clearly and unequivocally, in such wise (to put the matter a little paradoxically) that we can assure ourselves of what the writer's definition would have been, had he been able to formulate it. Ladd, as we have just now seen, attempts to exhibit the conscious by contrast with the not-conscious. We note, however, with some disquiet that—while psychology is to deal with the facts or phenomena of consciousness—it is 'we' and not these facts that are conscious, and that consciousness is made susceptible of degree, of less and more. If there is an universally realised difference between "external facts and facts of consciousness," in their status as data for scientific treatment, then surely the difference should be demonstrable from the face of the facts themselves, and 'we' who discern it should be left aside. Moreover, the facts, if they are data for scientific treatment, must surely either be or not be "facts of consciousness," and cannot vary their nature by more and less. Man is not more biological than amoeba, or the mountain brook less physical than Niagara.

Ladd's illustrations are therefore by no means free from objection,—so far, at any rate, as regards a science of "the phenomena of consciousness, as such." We must, however, take them as they stand, and see what Ladd makes of them when he comes to technical exposition.

A man of ordinary education and intelligence, looking back on his recovery from swoon or fever-crisis, would, no doubt, be likely to report a gradual restoration of 'consciousness:' by which he would mean an increasing awareness, an increasing realisation and appreciation and command, of himself and his surroundings. It is precisely this varied awareness, now,

[13] *Ibid.*, 30.
[14] *Ibid.*, 3.

that Ladd intends by his own technical use of the word 'consciousness.'[15] Every 'state of consciousness' with which a scientific psychology has to do is, he tells us, at one and the same time "fact of intellection, fact of feeling, fact of conation."[16] Intellection is evidently a mode of awareness: whether intellection mean that universal discriminating activity which has its root in the awareness of resemblance,[17] or whether it be taken to embrace the whole series of psychoses that runs from the abstract 'sensation' through perception and ideation to judgment.[18] In so far, then, as every state of consciousness involves intellection, consciousness is always, in this narrower intellectual sense, awareness. Feeling too, however, is a mode of awareness; it is our very being become aware;[19] and conation also is a mode of awareness, awareness of activity.[20] Every state of consciousness, therefore, is at once fact-awareness, value-awareness and activity-awareness, —the unitary awareness-resultant of three irreducible awareness-factors.[21]

Ladd thus obtains a starting-point for his system: but he has not adhered rigorously to his illustrations. For these states of consciousness, which are the primary data of psychology, while they all belong to some 'self' or 'I,' and indeed cannot be 'thought of' out of that connection, yet do not necessarily carry the reference to 'self' within or upon them; their consciousness is not necessarily a self-consciousness.[22] The convalescent of Ladd's illustrations, on the other hand, would assuredly maintain that 'he' became increasingly conscious, 'he' and not his 'states.' It is the observer who may become increasingly aware: the 'state' or 'fact' or 'phe-

[15] The two terms are not seldom used interchangeably. See, *e.g. op. cit.*, 166, 290, 293ff., 296f., 300, 310, 322, 328f., 331ff., 379, 422, 517, 523f., 530f., 636; Ladd and Woodworth, *Physiol. Psychol.*, 430, 512, 681, 685. They appear, indeed, from such passages as *Psychol.*, 11, to be strictly coordinate.

[16] *Psychol.*, 33, 58, 172, 264, etc.

[17] *Ibid.*, 33, 288ff., esp. 293.

[18] *Ibid.*, 93, 235, 251, 357, 430, etc.

[19] *Ibid.*, 170. Cf. the doctrine that feeling may precede or outlast ideation (181); the illustrations offered in support of a manifold of affective qualities (*e.g.*, 170); and the doctrine that feeling is an integral factor in (not merely a determinant of, or an influence upon) knowing (53, 510ff.; *Philos. of Knowledge*, 1897, 95, 124, 165f.).

[20] *Psychol.*, 83, 216, 219; *Philos. of Mind*, 1895, 87ff.

[21] 'Fact' must be understood, not in the sense of our own previous discussion [here not printed], but rather in the manner of common sense; cf. the illustrations, *Psychol.*, 17, 19, 50f., etc. For value, cf. *Philos. of Knowledge*, 124.

[22] *Psychol.*, 31f., 523.

nomenon of consciousness' is, as we saw, simply conscious, and cannot become increasingly or decreasingly what it essentially is.

We are, however, not yet at the end of Ladd's account. The state of consciousness which is to be the subject-matter of psychology must, he points out, do more than merely exist; it must become an 'object of knowledge.'[23] But it can become an object of immediate knowledge only by way of introspection or self-awareness;[24] and since this observing activity is itself a phenomenon of consciousness, the total state of consciousness, as object of psychological knowledge, is not just awareness, but rather self-awareness, awareness of awareness. The state remains unitary. If, however, we have recourse to logical abstraction, then the second awareness, the "phenomenon [of consciousness] known as fact," may be distinguished as 'content' from the activity of observation, the "knowing of the phenomenon [of consciousness] as object."[25] In this way the observer is brought within the conscious field, and degrees of consciousness are so far justified.

Still we are not at an end. The 'content' thus abstractly marked off from the activity of consciousness remains consciousness, and must therefore in its turn admit of the same distinction of activity and passivity. Sensations, ideas, feelings, conations may be regarded, passively, as 'content' of consciousness.[26] Their description and explanation make up half of the detailed psychological story. In the other half, consciousness (conscious content in the broader sense) is regarded actively: as intellection (or awareness of likeness and difference), as reactive feeling (mental tension, conviction), as attention.[27] Ladd is emphatic that "the task of a scientific psychology is as truly the description and explanation of the phenomena of consciousness, considered as forms of active functioning (of consciousness 'function-wise'), as it is the description and explanation of the particular qualities and quantities of the phenomena regarded as passive states (of consciousness 'content-wise')."[28] And he expressly applies this dictum to the "single state of consciousness, so far as

[23] *Ibid.*, 1f., 4, 7, 9, 32, etc.

[24] *Ibid.*, 9, 15, 523, etc. Introspection is also called self-consciousness and reflective consciousness.

[25] *Ibid.*, 32f.; cf. 37, 49, 289ff. Ladd varies in his use of the terms 'active' and 'passive:' see 46, 83, 96, 214, etc.

[26] *Ibid.*, 309. There seems to be inconsistency as regards feeling: cf. 19f, 163, 523.

[27] *Ibid.*, 288ff.; 308f.; 213, 289.

[28] *Philos. of Mind*, 86.

we can catch it and separate it from the stream of conscious life," *i. e.*, to what he has called the phenomenon known as fact, no less than to the "stream of conscious life in which every such state occurs," *i. e.*, to the total consciousness which includes the knowing along with the known.[29]

It appears, then, that Ladd operates with two distinct notions of 'consciousness:' the notion of consciousness as the sum-total of conscious states which make up the experience of an 'I,' and the notion of consciousness as the observing activity of this 'I' itself. If the states alone are conscious, there should be no more or less of consciousness: but Ladd, assimilating the conscious character of the states, their essential nature as awarenesses, to the conscious character of the observing self, is able to speak of degrees of consciousness at large. The complete datum of psychology, awareness of awareness, he regards as intrinsically unitary, but as separable by logical abstraction into awareness knowing and awareness known, activity or function and content of consciousness. The awareness known, the content-awareness, he then divides again, by the same abstraction and in the same terms, into activity or function and content. These divisions are confessedly artificial: there is no real line of division within the psychical fact: and we must suppose—since there seems to be no logical reason why they should not be repeated *ad infinitum*[30]—that they are carried only so far as is necessary for systematic exposition. Ladd himself justifies them, partly on the ground of convenience, but also, in the general portion of his treatise, because they indicate that psychology, the science of the phenomena of consciousness as such, is far more than description and explanation of merely passive 'content.'[31]

§ 3. In seeking to appraise Ladd's definition of the psychical, as subject-matter for a science, we shall confine ourselves to a few broad lines of criticism. And we begin by consider-

[29] *Psychol.*, 290f.

[30] Brentano (*PES*, i., 1894, 167) avoids the infinite regress by means of his *eigenthümliche Verwebung:* cf. my *Exper. Psychol. of the Thought-processes*, 1909, 47f. Ladd (*Psychol.*, 35) comments critically on Brentano's fourfold act, but does not appear to recognize the logical difficulty in which he is himself involved. I do not know that every logical regress is necessarily vicious. But in this case Ladd's system is not intelligible without the regress, while the nature of the regress itself is (as it seems to me) to be unintelligible.

[31] It follows, of course, that Ladd's 'physiological psychology' is, as psychology, only a portion of a science, and not an organized scientific whole: see Ladd and Woodworth, *Physiol. Psychol.* 381, 430, 542, 597, 625, 656, 664f.

ing (1) the place or position to which the definition assigns psychology within the group of the acknowledged sciences.

Observation, the immediate awareness of fact, seems in Ladd's view to be identical over the whole range of science. He draws no distinction of kind between inspection and introspection.[32] When, however, we turn to the relation between this direct method of acquaintance and the object upon which it is directed, we find that psychology is "peculiar, and indeed unique." For observation, being itself a fact of consciousness, merges into, or fuses with, the facts of consciousness observed; "it is separable, neither in reality nor in time, from the phenomenon observed as fact."[33] Our own discussion of psychological method must be postponed. Here we note only that, as regards the relation of its primary method to its subject-matter, Ladd marks off psychology, as unique, from all the other sciences.

The subject-matter of psychology, in what (for want of a better phrase) we may call its logical constitution, is also, for Ladd, unique. The state of consciousness, it will be remembered, is always, at one and the same time, fact of knowledge, fact of feeling and fact of conation. "This unity in variety, which belongs to all states of consciousness as such, is of unique character—and this, whether we lay emphasis on the unity that comprises the variety, or upon the variety comprised in the unity."[34] This uniqueness of constitution, exemplified by the single state of consciousness, is attributed by Ladd to the entire course of the mental life.[35]

Not only in logical constitution, however, but also in its self-determination, is the subject-matter of psychology unique. Descriptive and explanatory psychology leads us "to recognise a unique and self-active being" as, within limits, "interiorly

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[32] *Psychol.*, esp. 17f. Even the fact that psychical phenomena are "alterable—swiftly and largely—by the very act of attention which makes them objects of knowledge" seems to be paralleled on the side of inspection: see 18 (§ 3), 305f., 318f., 367f., etc.

[33] *Ibid.*, 32f., 319, 530, etc. Cf. *Philos. of Mind*, 160: "The knowing subject and the object are . . . woven into a vital oneness of being."

[34] *Psychol.*, 36, 172f.

[35] There is, namely, a "principle of continuity which gives its unique character to what we can observe of mental development. In all forms of organic physical evolution . . . the factors and stages of the evolution have some existence and value considered *in themselves*, as it were. But the case of mental development is not so. . . . Each factor, faculty, and stage exists for consciousness as in and of its own continuously flowing life-movement;" *ibid.*, 659f. The unity and variety of states of consciousness "are illustrations of this very principle of continuity as lying at the base of mental development:" 661.

determining, in a quite inexplicable way, its own course."[86] The presence among its data of this incalculable surd—whatever the limits of its operation may be—again separates psychology from the other sciences.

Here, then, are three points at which scientific psychology is wholly and necessarily out of touch with what should be its fellow-sciences. It is clear that the word 'science,' if it is to be retained at all, must be given an extraordinarily elastic meaning. In fact, the position which Ladd assigns to scientific psychology is a position, not within, but without the circle of the acknowledged sciences.[87]

(2) Ladd, however, has his own definition of science. "There is science," he says, "wherever there are ascertainable facts that may be described and explained in their relation to one another and to other classes of facts."[88] We must therefore consider, secondly, the nature of the 'facts of consciousness' that appear in his pages as the data of a scientific psychology.

Throughout his psychological writings Ladd is insistent that the phenomena of consciousness be regarded both content-wise, as facts of passive existence, and function-wise, as facts of activity.[89] The task that he sets himself is, accordingly, twofold: he must describe and explain both the content and the function of every typical psychosis; and, indeed, he must describe and explain the particular content as adequate vehicle of a particular function, and the particular function as correlated activity of a particular content. This of itself would

[86] *Ibid.*, 638, 662. We might append, as a fourth point, the fact that psychology sustains a "quite unique" relation to philosophy: *ibid.*, 638; *Philos. of Mind*, 71.

[87] Ladd can be sufficiently severe upon these 'acknowledged' sciences: *e.g.*, *Philos. of Mind*, 6ff. The point here, however, is that he distinguishes psychology by its three (or four) unique characters from the other sciences, not as seen by themselves, but as he sees them.

[88] *Psychol.*, 658. The 'affirm' of the following sentence should apparently be read 'deny.'

[89] Ladd observes that "a psychosis without content is equivalent to no psychosis at all; there are no phenomena of consciousness in general" (*Philos. of Mind*, 85; cf. *Philos. of Knowledge*, 200, and *Psychol.*, 30f.). Yet he frequently speaks, in the *Psychology*, as if consciousness were, after all, something 'apart from' and superadded to the 'actual psychic facts;' we read of 'conscious psychic activity' (214), 'conscious intellection' and 'conscious mentality' (296), 'the conscious mind' (305), 'conscious mental life' (321, 469), conscious feeling (583, 587), 'conscious ideation' (600), 'conscious conation' (623), 'conscious acts of will' (657), and 'conscious attention' (666f.). Similar phrases occur in Ladd and Woodworth, *Physiol. Psychol.*, 380, 463, 642, 671, 676, 679. These slips give further evidence of the instable nature of Ladd's concept of consciousness.

be a sufficiently difficult programme; but Ladd is hampered in his undertaking by his double use (to which we have already referred) of the term 'consciousness,' which means both 'my' awareness and also the awareness intrinsic to a psychosis as such. Consider, for example, that "convenient abstraction," the sensation. Every sensation is at once active and passive, a "psychical activity" and a passively received "impression." As active, it should be actively aware, actively cognisant of the "quality belonging to the object of sense." In Ladd's account, however, it becomes, even while regarded as intrinsically active, an "item of information" to 'me;' 'my' sensations "become objectified, as my feelings and thoughts cannot, in the form of qualities of perceived *things*." The function which should be the sensation's own is thus transferred, from the sensation, to the 'me' to whom all sensations belong.[40]

In this case, then, there is loss to be noted; in the case of primary intellection, on the other hand, we have a superfluity. Intellection, as actively discriminating consciousness, is "within," is "an integral part of," every state of consciousness, so that a concrete psychosis is by its very nature self-discriminating and self-discriminated. Yet if I make a state of consciousness the object of my regard, discriminating consciousness is found to "accompany" the now passive fact. It is perhaps intelligible that 'my' discriminating consciousness should be needed to discern the discriminating activity which is native to the psychosis (though it must be remembered that, as such, these two activities are identical); but it is surely not clear how this attendant discrimination of mine helps toward the discernment of the already self-discriminated 'content.' Two discriminating activities, of precisely the same kind, are here set to work in circumstances where it seems that one would be sufficient.[41]

In still other cases, the duplication of consciousness leads to sheer logical confusion. Suppose, for instance, that I, by way of primary attention, make a state of consciousness the passive object of my consideration: primary attention is then the degree of psychical energy "expended upon" the different aspects or moments of the state. Yet every mental state, as active, has its own degree of this same psychical energy, upon which attention, in its present turn, is constantly dependent. Ladd tries to save his logic by the remark that these two statements "only serve to approach the same truth from different

[40] *Psychol.*, 93ff.
[41] *Ibid.*, 33f., 288.

sides." But the truth, as he leaves it, seems rather to be a matter of alternatives.[42]

Difficulties of this sort recur again and again to baffle the student of Ladd's psychology. The 'facts of consciousness' with which the system deals are both ambiguous and instable. They are active and they are passive, they are forthputtings of a mind and data of a science, they are conscious and I am conscious of them. Moreover, they play their systematic parts with so little regard to omission and repetition and contradiction that the plot of the play tends to be lost.

(3) This plot itself, we must add,—the systematic working up of the psychological materials,—shows a like ambiguity. Ladd declares, for example, that the mental life, from its very beginnings, carries the plain promise of a plan, so that "no *science* of the life of mind is possible without recognising the presence of final purpose;" yet he affirms also that "in attempting a scientific account of the mental life psychology is justified in laying emphasis, at first, upon the passive and, as it were, externally determined side of the total development," as if no plan were visible or operative.[43] Here is no distinction of function and content within consciousness, but the recognition of two radically different psychological attitudes toward mind in general. The same attempt to have things both ways is seen in the treatment of the faculties: Ladd, in all literalness, both accepts and rejects the doctrine of faculties. He speaks, in formal reference, of the 'so-called' faculties; he explains that the term 'faculty' is both futile and dangerous; and still he employs the concept, repeatedly and constructively, in his psychological exposition.[44] And, as a final instance, the same criticism holds of Ladd's treatment of mental composition. He makes free use of the terms 'mix-

[42] *Ibid.*, 74f., 78, 83, 621. On 'psychic energy' (of which Ladd gives no definition) see 39, 41, 44, 64f., 78, 83, 132, 261, 386, etc.

[43] *Ibid.*, 266f., 286f., 414, 664f., 668; *Philos. of Mind*, 203; *Philos. of Knowledge*, 473, etc.

[44] *Psychol.*, 33, 45f., 49ff., 60, 288, 317f. 380, 409f., 455, 490, 612, 659, 664. In particular: (1) Ch. iv. is headed "The So-called 'Mental Faculties.'" Ladd's use of 'so-called' is a mannerism, which began perhaps as a defensive reaction against the possible charge of looseness of language. Since it has come with him to mean anything from 'what is ordinarily called' to 'what is falsely called,' it does him little service. (2) The adjectives 'futile' and 'dangerous' are not too strong. For we are told (51) that words like 'faculty' do not explain; that they help but little in classification; and that their use, however guarded, is "likely to occasion. . . . a generally inadequate and misleading account of the development of mental life." (3) For the seriousness with which the concept is taken, see such passages as 380, 490.

ture' and 'blending' and 'fusion' and 'association,' while yet he assures us that this language is figurative, adopted only for the avoidance of "almost unending periphrases." But is taste, or is it not, a complex of "gustatory, olfactory and tactual elements?" And is there, or is there not, in stereoscopic vision, a 'combination' of visual with tactual and motor sense-complexes? The reader looks in vain for a single periphrasis to clear his mind, and is forced to the conclusion that Ladd's logic is trying to say both Yea and Nay of the same subject-matter.[45]

(4) Where both data and applied logic are thus ambiguous, it is perhaps unnecessary to urge that the resultant is not an organised system. Since, however, we are presently to discuss the working concepts of 'function' and 'content' in their general applicability to the subject-matter of psychology, we cannot afford to neglect this formal point of criticism. We note briefly that Ladd's system breaks bounds on every side.

Ladd starts out with the threefold unity of intellection, feeling and conation, all of which 'moments' or 'aspects' of consciousness are to be taken both as content and as function. These are the material postulates of his psychological system. But they prove to be inadequate. The study of intellection, for instance, brings us in time to the problem of recognitive memory; and here our scientific advance is arrested; recognition is "a form of mental reaction *sui generis*, which, while depending upon conditions . . . , has still a unique character that transcends the conditions on which it reposes."[46] In like manner the study of feeling brings us to the feeling of obligation and the sentiment of moral approbation or disapprobation. "These two forms of moral feeling are unique. Why they arise in the individual, and why they have that nature and connection with each other, and with the development of intellect, which they actually have—these are questions which psychology [even as explanatory] cannot answer."[47] Lastly, the study of conation brings us to volition, where "psychologically considered, it is no less true that I will the influential ideas, feelings and desires, than that the ideas, feelings and desires influence the final 'I will.'"[48] So that there

[45] The following sentence (*ibid.*, 235) is characteristic: "The introspective and experimental analysis of modern psychology cannot be abandoned, because, in spite of repeated explanations, some readers will persist in misunderstanding our necessarily figurative terms." Cf. 18f., 23, 37, 38f., 89ff., 94, 102f., 106, 115, 118, 132, 141ff., 146, 160, 180, 186, 209, 253ff., 318f., 323, 349, etc.

[46] *Ibid.*, 382, 397, 399, 401.

[47] *Ibid.*, 581ff.

[48] *Ibid.*, 618f., 625f., 635, 638.

is no single aspect of mind, no single strand of mental development, for which the complication of function and the compounding of content are sufficient. The system is disrupted on the side of intellect, on the side of feeling, and on the side of will.—

There can be little question, then, that Ladd has failed to erect a science of psychology on the basis of a special class or department of "phenomena of consciousness." We looked at his 'so-called' science in its general status and relations, and we found it to stand apart from all acknowledged sciences. We glanced over the contents of his exposition, and we found that his programme has not been carried out. We considered the logic of his system, and we found it no less ambiguous than the materials to which it is applied. We have now examined the result to which his systematic labours have attained, and we find him hopeless of a system. It remains to ask whether his failure is the failure of a particular author, or whether it is the failure of his psychological position 'as such.'[49]

[49] The systems of Angell and Judd can be less certainly appraised, partly because they have so far been carried out only at the text-book level, and partly because their writers are more interested in function and genesis than in content. We may, however, note the following points.

For Angell, psychology is the science of consciousness, and consciousness is awareness (*Psychology*, 1908, 1, 222, 366, 442; cf. 5, 150, 185, 199, 228, 246ff., 337, 364f., 370, 373, 383ff., 399, 426ff., 430, 441ff., 444, 446). On its subject-side, consciousness is the observing activity of a self; 'we' are immediately aware of thoughts and feelings, of perceptions, images and emotions (2, 442f.; cf. 84, 302, 401, 408, 431, etc.). In its object-half, or on its content-side (content being taken in the broader sense), consciousness shows the two aspects of structure or content (in the narrower sense) and function (*e.g.*, 201). States of consciousness are functionally unitary, but by logical abstraction may be divided into cognitive and affective (302, 436f.). [The position of attention is ambiguous. Attention appears to be a function of subject-consciousness, with a structure on the side of object-consciousness; there is apparently no discriminable attentive function within object consciousness: 80ff.] Both the cognitive and the affective functions are, in their own right, modes of awareness: the cognitive are informative (109, 170, 198, 201, 222, 248), the affective are evaluative awarenesses (302, 320, 322, 327, 378, 382). In general outline, therefore, Angell's system is very like Ladd's.

For Judd, likewise, psychology is the science of consciousness (*Psychology*, 1917, 1, 5, 10, 12, 38, 145, 309; behaviour is to be studied for the understanding and explanation of consciousness), and this consciousness is awareness (2ff., 6, 12f., 142, 238, 329; cf. 27, 65, 73, 160f., 166, 169, 183, 190, 212, 233, 246, 270, 276, 291, 345). Awareness, however, is always the awareness of 'someone;' it is 'one' or 'the observer' or 'the individual' or 'the child' that is conscious (1, 141f.,

The Functional Systems

§ 4. We pass, accordingly, to a consideration of the features common to functional systems in general.[50] There seem to be four main tenets or tendencies which we may regard as characteristic of the school.

155, 169, 189, 241, 272, 301, etc.). 'Conscious processes,' which are classified according to nervous processes (64ff.), show the familiar duality. Over against sensations (73, 188), which are discussed content-wise, stand the attitudes of feeling and attention (66f., 146ff.), which are discussed functionwise. Perception includes, on the side of function, the activities of fusing, locating, distinguishing, recognizing, in a word of relating sensations (163, 166, 169, 186, 189, 191), and on the other side the product or resultant of these activities, the percept or relational fact, a content of a higher order (169, 175, 186f., 189, 191f.). The content of memory consists in images, which are primarily substitutes for sensations and percepts (241ff.), and its activity consists in recall according to the laws of association (244ff.). Ideation furnishes us with contents of a still higher order, difficult of precise description (246), and with such activities as conception, abstraction, generalization, judgment and reasoning (263ff.). Lastly, in voluntary choice we have a personality, an organized whole of mental activities (308), directed upon 'ideas' which are themselves organized "composites of experience" (306, 309).

There remains the question of the 'someone' who is thus in manifold ways and degrees aware. Judd's teaching is that we must conceive the knowing self, to which the study of conscious processes leads us, as an unitary being,—a 'conscious being' which, like the 'living being,' is an organized unity (274ff.; cf. 263). The self possesses and, by unifying, modifies conscious states (274). It is, however, difficult—since the terms 'self' and 'personality' are used interchangeably—to reconcile the statement that "the self is a being which perceives and forms concepts" (274) with the statement that "personality is the name of that individual nature which has been developed out of the play and interplay of impressions and instincts and conscious comparisons and imaginations" (308). See p. 521 above.

[50] This is not the place for a bibliography of functional psychology. I give a few early references, and name two papers of objective import.—W. James, "On Some Omissions of Introspective Psychology," *Mind*, ix., 1884, 18f.; *The Principles of Psychology*, i., 1890, 478; H. Ebbinghaus, *Grungzüge der Psychol.*, i., 1897, 161ff. (i., 1905, 176ff.; changed by Dürr, i., 1911, 175ff.); E. B. Titchener, "The Postulates of a Structural Psychology," *Philos. Rev.*, vii. 1898, 449ff.; "Structural and Functional Psychology," *ibid.*, viii., 1899, 290ff.; C. A. Ruckmich, "The Use of the Term '*Function*' in English Text-books of Psychology," *Amer. Journ. Psych.*, xxiv., 1913, 99ff.; K. M. Dallenbach, "The History and Derivation of the Word 'Function' as a Systematic Term in Psychology," *ibid.*, xxvi., 1915, 437ff.

Angell remarks (*Psychol. Rev.*, xiv., 1907, 63) that 'structural psychology was the first to isolate itself.' In a broad sense, that statement is true; functional psychology claimed the whole field. In historical detail, however, things are less simple. There had already appeared in America the two overtly functional systems of J. M. Baldwin (*Handbook of Psychology*, 1889-91) and Ladd (1894). Moreover, it was as early as 1887 that Ladd, in his *Physiological Psychology*,

(1) The distinction between the 'activity' or 'function' and the 'content' or 'structure' of consciousness is recognised, explicitly or implicitly, by all the psychologists of function. It reflects, of course, the biological distinction of organic function and organic structure, of physiology and morphology. We say 'biological,' in deference to current usage: in point of fact, the distinction is rather technological than scientific. It belongs of right to the great technology of medicine; it was accepted, and found useful, by human physiology; it remained serviceable in the beginnings of comparative physiology. On the strict ground of science, it is no longer adequate to our knowledge;[51] and if it still persists in biological textbooks, the reason is again largely technological: the distinction recommends itself for pedagogical purposes. Even in medicine, its encouragement of a rigid specialisation lays it open to criticism.

There is, nevertheless, in the 'biological' context, a certain appeal to common sense, a certain plausibility, about this correlation; heart and lungs and hand and brain, regarded as machines, are obviously 'adapted' to the 'ends' which they subserve. We search in vain for any such plausible coördina-

sought deliberately to subsume the whole body of experimentally observed facts to a functional view of 'the nature of mind.' So that functional psychology had been a little uneasy, a little self-conscious, for a whole decade before the catch-word 'structural' came on the controversial scene. And the first note of revolt (E. W. Scripture's *New Psychology*, 1897) was struck in the physical, not in the biological key. Scripture's attempt proved abortive, partly because the physical concepts of time, space and energy were inadequate to the psychological data, and partly because he represented his new psychology as a straight development from the old: "there is no difference in its material, no change in its point of view" (453). 'Structural' psychology, as its name implies, recognized the existence of a correlative psychology of function; it isolated itself only to the degree that it demanded equal rights for content and activity, and by so doing protested against the autocracy of function. In the 'biological' atmosphere of its time the phrase did good controversial service. If the view of my book is accepted, both 'functional' and 'structural,' as qualifications of 'psychology,' are now obsolete terms.

[51] As appears, *e.g.*, from such works as H. S. Jennings' *Behaviour of the Lower Organisms* (1906), and J. S. Haldane's *Organism and Environment as Illustrated by the Physiology of Breathing* (1917); cf. also B. F. Kingsbury, "The Fitness of Organisms from an Embryologist's Viewpoint," *Science*, N. S. xxxviii., 1913, 174ff.; "The Interstitial Cells of the Mammalian Ovary; Felis domestica," *Amer. Journ. Anat.*, xvi., 1914, 79; "The Development of the Human Pharynx," *ibid.*, xviii., 1915, 374ff. It is obvious that those who identify biological phenomena with physicochemical processes have no need of the concept of function (see *e.g.*, J. Loeb, *Forced Movements, Tropisms and Animal Conduct*, 1918); but this view is not here under consideration.

tion of conscious content and conscious function. The "forms of receptivity," according to Ladd, stand in "bewildering and unclassifiable variety" over against the "relatively few forms of organising activity displayed in all mental states."[52] If content is to be made adequate to function, then (as in the instance of the 'representative image') function must be read into content, so that the content becomes an hypostatised function.[53] Nowhere is the attempt made to show that the strictly observable 'contents' are the suitable vehicles or instruments or substrates of the various modes of psychical activity.

Why, then,—it is natural to ask,—why, and with what hopes, did psychology submit itself at all to 'biological' guidance, and seek within its subject-matter for the analogues of organic structure and organic function? The answer refers us to historical conditions. Historically viewed, the functional psychology of which we have taken Ladd to be representative is a plea in avoidance offered before the court of science on behalf of empiricism. The continually growing body of experimental facts, menacing even while unorganised, had to be reckoned with; and the distinction of function and content afforded a means whereby it might be duly subordinated to the empirical system. If the worst should come, and experimental psychology should be able presently to organise itself as an independent science, still the empiricist had gained time, a breathing-space for adjustment, and had gained also a set of working concepts by whose aid the break might, so to say, be eased and graded. What he expected, however, as is clear from all of Ladd's work, was a new lease of life for the traditional empiricism. The particular line which his reconstruction followed was settled for him by the status of science at the time. Not mathematics, not physics, was the characteristic modern 'science,' but biology; and if biology thought and spoke in terms of structure and function, psychology need not scruple to think like thoughts and to use like language. So there arose a new empirical psychology, in all essentials at one with the old, but more or less effectively disguised under the cloak of contemporary science. After the event, we can see that psychology borrowed in haste, without assurance that the loan could be turned to properly

[52] *Psychology*, 53ff., esp. 57.

[53] *Ibid.*, 234ff., 244ff., 376. D. S. Miller ("The Confusion of Function and Content in Mental Analysis," *Psych. Rev.*, ii., 1895, 536) meets the difficulty by a reference to the unlikeness of physical cause and physical effect. But content does not stand to function as cause to effect, and physical causes and effects are alike energetic.

psychological account. We can see, further, that the biology to which psychology appealed was not in truth a scientific biology, but a biology conventionalised and popularised. At the time, nevertheless, it was only natural that the contents and functions of this new empirical psychology should seem to bear the authentic stamp of science.

(2) In the second place, functional systems are disposed to correlate the phenomenon of consciousness with unreadiness or inadequacy of the nervous system; "consciousness is only intense," remarks James, "when nerve-processes are hesitant."[54] As soon as the organism is confronted by a problem which its existing neural organisation is unable to meet, consciousness steps in to dispel the perplexity; "straightway appears consciousness, with its accompanying cortical activities, taking note of the nature of the stimulus and of the various kinds of muscular response which it called forth."[55] The point must be taken, it would seem, with a certain reservation: for it is not unreadiness or inadequacy in general that is responsible for the appearance of consciousness, but rather such unreadiness and such inadequacy as require, within the biological limitations of the particular organism, only a certain additional prompting or urging to change into readiness and adequacy. With this reservation more or less expressly made, some authors, like Angell, declare in round terms that "conscious activities emerge at the point where reflex acts are found inadequate to meet the needs of particular situations;" "if the reflexes and the automatic acts were wholly competent to steer the organism throughout its course, there is no reason to suppose that consciousness would ever put in an appearance."[56] Others, like Dewey, argue that difficulties in

[54] *Principles*, i., 1890, 142. James, although he favoured a functional as opposed to a structural psychology ("The Energies of Men," *Philos. Rev.*, xvi., 1907, 2), does not give us in the *Principles* a system of functional psychology. He gives us a work on the principles of knowledge, written from a psychologistic standpoint. If his volumes are read with this interpretation in mind, the critics' charge of lack of plan (which James repelled: *Text-book*, 1892, iii. f.) will be found groundless.

[55] Angell, *Psychology*, 1918, 64.

[56] *Loc. cit.* Angell confines his discussion, in these passages, to the human consciousness. The question 'why' the human infant should be limited to a certain group of inherited coordinations he hands over to 'the biologist' for answer. So one might hand over many other questions: 'Why,' since in the last resort the environment is for all organisms the same, should any infant embark on a career for which his inherited coordinations are insufficient? and 'why,' having thus embarked, should he again meet with limitations to his conscious powers? and so on. The biologist, in so far as he is man of science, will maintain a discreet silence.

the way of nervous conduction heighten the clearness of consciousness, but do not commit themselves outright to a theory of its first appearance.[57] There are thus differences of detail.[58] It would, however, be widely agreed that, at any rate in the case of man, the 'condition' of consciousness is a temporary and corrigible imperfection of nervous organisation.[59]

A view of this sort seems, indeed, to be logically bound up with the view that consciousness is primarily and actively a matter of function, and only secondarily and passively a matter of content. For if consciousness is to do us some organic service, it will find its natural opportunity in some defect of our given (non-conscious or extra-conscious) organic equipment; and this, in the categories of our current thinking, means some defect in the functional capacity of the brain. Positive evidence, however, is found in the experience of every adult. We have learned to write, to use a typewriter, to ride a bicycle, to play a piano, to drive a motor car; and we know that, as the original problems cease to baffle us, and the skilled movements become habitual, the need of conscious 'control' grows less and less; we find that consciousness 'intervenes' less frequently and less imperatively, until it may finally disappear. Could there be more convincing proof of the instrumental function of consciousness? and is not the

[57] So at least I understand these discussions. See J. Dewey, "The Theory of Emotion," *Psychol. Rev.*, i., 1894, 553ff.; ii., 1895, 13ff.; "The Reflex Arc Concept in Psychology," *ibid.*, iii., 1896, 357ff.; "The Psychology of Effort," *Philos. Rev.*, vi., 1897, 43ff.

[58] The same general idea has been expressed in various places by W. McDougall. See, *e.g.*, "A Contribution towards an Improvement in Psychological Method," *Mind*, N. S. vii., 1898, 159ff.; "On the Seat of the Psycho-physical Processes," *Brain*, xxiv., 1901, 607f.; "The Physiological Factors of the Attention-Process," *Mind*, N. S. xi., 1902, 341f.; *Physiological Psychology*, 1905, 59f.

It is possible that Ladd is adopting a like view when he makes conation or conscious striving "the most fundamental of all psychic phenomena" (*Psychol.*, 219). Ladd and Woodworth, however, combat the theories of Dewey and McDougall (*Physiol. Psychol.*, 1911, 610ff.).

[59] Judd ("Evolution and Consciousness," *Psychol. Rev.*, xvii., 1910, 77ff.) forms a marked exception to the rule. Consciousness appears only when the organism has attained a certain stage of complexity of inner organization (94). The question of first causes—where this consciousness comes from—is not raised (92). Having appeared, however, consciousness functions as a centre of reorganization (93f.) The more highly organized, *i.e.*, the more self-sufficing the organism, the greater is the rôle and the higher the development of consciousness (80f., 88). In a word, consciousness is at once the product, the index, and the cause of organization.

formation of a habit typical of the general course of the mental life?[60]

We must here meet the psychologist of function on his own ground; we must, that is to say, grant him his assumption that consciousness, the subject-matter of psychology, is awareness. The question then arises whether our conscious life is, as a matter of fact, nothing more than a recurrent grappling with problems. Are we aware only when and in so far as we are searching, reconciling, reducing to familiar terms, trying to understand,—or whatever other words there may be that imply the setting of a problem and the urgency of its solution? Surely we are not; surely there is a contemplative as well as an acquisitive awareness. We are not always learning something new, or practising this new until it becomes old. There are times when we sun ourselves, as it were, in the full light of consciousness,—times when, without any "felt necessity for further nicety of adaptation to surroundings,"[61] we are none the less keenly and competently aware. There is a consciousness militant, but there is also a consciousness triumphant: more than that, there is a consciousness that, in the midcourse of its campaigning, rests quietly upon its arms, and surveys the terrain it has occupied. A narrowly instrumental theory of consciousness comes very near to a *reductio ad absurdum* of the biological 'struggle for existence.'

(3) The two characters that we have so far discussed—the distinction within consciousness of content and activity and the assignment to consciousness, especially in its active phase, of a value for organic survival—lie on the surface of the functional systems. Underneath, always and everywhere, runs the steady current of teleology. It is one of the ironies of the history of science that the great biological generalisation which was to free us of teleology in our study of the phenomena of life should give rise, in post-Darwinian days, to an unbridled license of teleological 'interpretation.'[62] Biology

[60] Judd is here consistent. "Too often the psychology of habit has been guilty of the statement that habituation leads to unconsciousness. This is not the case" (*Psychol.*, 1917, 207).

[61] Angell, *Psychol.*, 1908, 74. Note the implication of the phrase! Consciousness, which might on Angell's terms be described, roughly, as the "felt process of adaptation," here anticipates its own function.

[62] "If we apprehend the spirit of the 'Origin of Species' rightly, then, nothing can be more entirely and absolutely opposed to Teleology, as it is commonly understood, than the Darwinian Theory:" T. H. Huxley, "Criticisms on 'The Origin of Species'" (1864), *Lay Sermons, Addresses and Reviews*, 1887, 264. It is a mind "stored with the choicest materials of the teleologist that rejects teleology,"—so J. Tyndall speaks of Darwin (*Report B. A. A. S., 1874*, 1875, lxxxvi.). Cf. E. du Bois-Reymond, *La Mettrie*, 1875, 23.

has suffered, and is still suffering, from that license. Biology, nevertheless, again furnished psychology with the obvious scientific parallel. Biologists spoke in terms of final cause, the whole system of biology was pervaded by teleology, and psychology again had no call to be more scientific than science. We have seen the outcome in Ladd's thinking: a teleological activity of organisation plays upon a causally (or quasi-causally) determined content, to the inevitable confusion of the concept of consciousness which is fundamental to his psychology at large. We find a like ambiguity in Angell's book. Mind, as we have said, there "seems to involve the master devices" through which the "adaptive operations of organic life may be made most perfect," and "the real business of consciousness is to be sought amid the adaptive responses of the organism to its life conditions."[63] But when we go in search of master-devices and real business we are told that the basal distinction is that "between certain kinds of nervous activity overtly involving consciousness . . . and certain other kinds not overtly involving it;" consciousness is only the "index" of "problem-solving adaptive acts." Nay more: if we speak "as though mind might in a wholly unique manner step in and bring about changes in the activity of the nervous system," we are employing "convenient metaphors," "a convenient abbreviation of expression," which must not blind us to "the fundamental facts which lie behind."[64] Such are the logical sacrifices that teleology demands of her children!

May it not be, however, that the fault lies with the children? May it not be that the psychologist of function takes his teleology a little carelessly? It has come down to him from the older empiricism; it is guaranteed by philosophy and technology; it is justified by biological example. Small wonder, then, that he should slip easily, even heedlessly, into the teleological attitude! But are we on that account ruthlessly to banish teleology from a scientific psychology?

One could wish, certainly, that the functional systems were somewhat less confident of their position. For it is one thing to affirm broadly that "the stream of consciousness appears . . . as a current designed from the beginning . . . to the fit performance of a certain work,"[65] and quite another thing to build psychological facts and laws, the details of psychology, into a coherent system of means and ends. The

[63] *Op. cit.*, 8, 95.
[64] *Ibid.*, 59f., 300.
[65] Ladd, *Psychol.*, 668f.

teleology which is used in psychology as a tool of scientific construction is, as a matter of fact, both clumsy and double-edged. The psychologist may insist on final causes as he will, but he never makes them adequate to the refinement of observation;[66] and he may answer any number of Whys, but he is still faced by unanswerable Why-nots that throw doubt upon his positive explanations.[67] Yet we must insist that final causes, if they are at all recognised in psychology, be recognised primarily for psychology's sake. It is not enough to infuse just so much of teleology into the psychological system as shall orientate us toward ethics or history, or place us within a generally biological context, or blur the difference between 'pure' and 'applied' science. Psychology has its own claims in behalf of every item of its subject-matter.[68]

There is, however, no reason to suppose that these claims could be met by any teleology, even the most self-conscious and the most persistent. The whole history of science argues to the contrary. We may freely grant that teleological ideas have the occasional heuristic value which certain investigators

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[66] "Fortunately for science," writes Judd (*Psychol.*, 90f.), "there have been a few cases in which the same person has been able to observe directly both the normal color sensations and the partially color-blind series." Fortunately for descriptive science, truly: but how fortunate for psychology in Judd's sense? How has the development of red-green vision aided man in the struggle for existence(4)? or what has man gained by the "unique compromise process"(92) which gives rise to the purple sensation? These and like questions are not touched.

[67] "It is because we have . . . no special organ affected by weak currents of electricity that men overlooked for so long a period both the prevalence of forms of electrical energy and the close relation between light and electricity" (Judd, *op. cit.*, 72). But, granted that the facts are as stated and granted that this furtherance of knowledge is useful, why have we not the special organ?—for it is surely evident that biological conditions, which have produced the 'electric fishes,' are also competent to produce an electrical sense-organ in man.

Again: "We do not have microscopic eyes like the fly. Nor . . . do we have distance vision like the eagle's. . . . The range of human vision has been determined by the range of possible human reactions" (132); cf. W. H. Hudson, *Idle Days in Patagonia*, 1893, 183 f. But has not man extended his reactions by microscope and telescope, and are not tools (249) sensory as well as motor? Cf. H. Spencer, *Principles of Psychol.*, i., 1881, § 164, 365; O. Wiener, *Die Erweiterung der Sinne*, 1900.

[68] How utterly this subject-matter may drop out of sight, under the prepossession of teleology, appears in the discussion by A. E. Taylor, *Elements of Metaphysics*, 1903, 306f.

have claimed for them.[69] It remains true, notwithstanding, that these same investigators have only by exception had recourse to teleology, and have not been encouraged to adopt it as a guiding principle of research. It remains true that final causes have long been banished from the domain of the older sciences, and that they flourish only where (and in proportion as) exact knowledge is wanting. It remains true that interpretation by means and end tends to close enquiry and thus to bar the progress of scientific knowledge. In particular, it is highly significant that biology, after full trial of teleological principles, is in these latter days resolutely turning away from final causes to the laborious planning of experiments and the patient accumulation of observed facts, —so that 'evolution' and 'heredity' and 'adaptation,' once the means whereby we conjured ends, are now coming to be mere descriptive labels for laboratory note-books. The whole history of science thus goes to show that teleology is essentially non-scientific. And, if that is the case, there can be no room for "teleological import" within a "*science* of the life of the mind."[70]

(4) Finally, this teleological attitude threatens the stability of psychology as an independent branch of knowledge. The psychology of the functional systems appears as transitional, as a stage either upon the difficult ascent toward philosophy or upon the level road that leads to various application; always it appears as a half-way house on the journey to something else, and not as an abiding-place. The individual psychologist may look forward to the one goal or to the other, or may perhaps keep both in view; that is a matter of temperament and training. It is at any rate characteristic of the school that they are not content to rest in psychology. Their psychologising, with whatever pains and seriousness it is done, seeks to transcend itself, as if in the last resort it were done not for its own sake but for the attainment of some foreign end.

Ladd, for instance, bears witness on the side of philosophy. "The problems of philosophy," he tells us, "all emerge and force themselves upon the mind in the attempt thoroughly to comprehend and satisfactorily to solve the problems of a scientific psychology; and the attempts, along the different main lines of research in psychology, to deal scientifically with

[69] E. Mach, *Die Analyse der Empfindungen*, etc., 1900, 60f. The instance cited is not physical but biological. I do not find in the *Erkenntnis und Irrtum* (1906) any acknowledgment of the heuristic value of teleology in physics, though the book as a whole contains a good deal of teleological thinking.

[70] Ladd, *Psychol.*, 668.

its problems all lead up to the place where this science hands these same problems over to philosophy."[71] Psychology is therefore of value as a propaedeutic to philosophy, in so far as philosophy is too difficult and too intricate to be approached directly and without an introductory easement; but the psychologist cannot, in scientific status, compare with the physicist or chemist or biologist. On the contrary! The longer he works in psychology, the more clearly does he confess his inability to tackle his problems at first hand; and the more able he is, the briefer will be his psychological apprenticeship to philosophy.

Judd, on the other hand, lays stress on the importance of psychology for the understanding and consequent control of human life and human institutions. He devotes a chapter of his general *Psychology* to the maxims of 'mental hygiene,' by following which the individual may attain to the highest level of organisation, "when mental development becomes a matter of voluntary control." He devotes another chapter to the institutional applications of psychology, its usefulness in literature and the fine arts, in the social sciences and anthropology, and especially in education. He recognises, to be sure, that psychology's relation to philosophy is "closer than that of any of the special sciences," but the recognition is formal and carefully qualified. The main interest of the study of consciousness is that it gives the key to human nature, and thus enables us intelligently to guide the course of the individual and of society.[72]

There is no need of further examples, but there is great need that the reader clearly understand what these particular examples are meant to show. The point here at issue is not that the author of a scientific text-book should shut himself up within the four walls of his specialty, and look neither above nor about. Where we find such narrowness of vision, we are likely to find also confusion of thought. The point is rather that we note a marked difference of emphasis between text-books of functional psychology and text-books of other sciences. A text-book of physics will discuss various types of engines and machines, but it will discuss them as illustrative of physical laws. A text-book of physiology may discuss various types of pathological phenomena, but it will discuss them in connection with physiological methods and

[71] *Philos. of Mind*, 1895, 73; *Psychol.*, 12. Cf. J. R. Angell, "The Relations of Structural and Functional Psychology to Philosophy," in *The Decennial Publications of the University of Chicago*, iii., 1903, 55ff.; *Psychol.*, 1908, 9ff.

[72] *Psychol.*, 1917, 314ff., esp. 324, 344ff.; cf. 10, 208, 268, 299f., 309.

physiological principles. The text-books of functional psychology, on the other hand, tend—it is true, in varying degree —to make of psychology either an introduction to philosophy or an aid to individual and social welfare. In so far as these tendencies prevail: in so far as functional psychology, in its exposition of psychology proper, goes out to meet the problems of philosophy or of our customary human life: in so far there is real danger that the pains and seriousness which are the due of psychology as science are withdrawn from psychology and expended in those other fields.—

These, then, seem to be the four main characteristics of the functional systems. The subject-matter of psychology is duplicated, though function is preferred to content; consciousness is a solver of problems; the whole course of the mental life is regarded teleologically; and psychology is written as a preface to philosophy or to some practical discipline. We have already indicated that these characters are not logically coördinate. A logical arrangement might, however, have appeared to prejudge the case, whereas our topical presentation has required that every character be discussed on its individual merits. In any case it should now be plain that functional psychology has its roots in the Aristotelian empiricism, and that while it has taken color of modernity from the surrounding sciences it has not adopted the modern conception of science itself. For this reason Ladd's deliberate and sustained effort to maintain the continuity of psychology as science was foredoomed to failure. His loyalty to the past is incompatible with his open-mindedness toward the future. He recognises, frankly if a little anxiously, the gradual emergence of the scientific problem, yet he cannot bring himself to discard, even in what he defines as science, the constructions of prescientific thinking. He is thus betrayed into an illogicality which, in any other context, he would have been among the first to discern.

CHURCH HISTORY AND PSYCHOLOGY OF RELIGION

By Pierce Butler, The Newberry Library, Chicago

The following paper is written by a student of church history, not by a psychologist. It attempts to emphasize, by citing certain specific examples, the importance and necessity of testing historical conclusions by the norm of psychological law. Furthermore, it ventures to criticise the present work of religious psychologists from the historian's point of view, and to suggest certain lines of investigation and verification in which he seems most to need professional assistance.

I

Formal instruction in religious psychology is given in most of our better theological seminaries; and more than this, it has become a commonplace in clerical circles that theologians ought to study psychology. There is thus an enormous amount of desultory reading of psychological treatises and text-books in ministerial studies. Unfortunately, however, a greater part of this study goes no further than a genetic investigation of the religious consciousness in a normal mind. It is Starbuck and his disciples whose writings are read rather than James, of whom indeed I believe it must be said that he writes altogether above the heads of many clerical readers. As a result, there is little reaction upon historical conceptions from the newer scientific knowledge of the human mind; only perhaps in his notions of the Crusades, or of the great religious revivals, is the student of ecclesiastical history directly influenced by his psychological readings. In these two matters his study of the mob-mind in such works as the *Social Psychology* of E. A. Ross inevitably becomes an element in the formation of his historical judgments.

But it is neither the religious experience of normal man nor great popular enthusiasms that most concern the student of ecclesiastical history; usually, the most difficult problem in his investigation of any period must be the understanding of the mind of a religious genius whose dominance over his fellows forms the figures which are printed upon the canvas recording the religious experience of the age. The reactions of normal men and women upon the teachings and examples

of a leader constitute the warp and woof of the fabric, but this must usually remain unwritten and unwritable history; certainly it can never be reconstructed, be it never so tentatively, where one's understanding of the leader is distorted or untrue,—as it is quite certain to be when sympathetic insight into the mental processes of the religious genius is totally wanting.

Much time and labor are spent by modern students of the formative centuries of ecclesiastical history in tracing out, by philological evidence alone, influences and origins of constituent ideas and practices in various systems. Indeed, to some writers the ideal history of the course of religious thought seems to be a genealogical tree of theological "schools." Almost every German scholar who has attempted a *Dogmengeschichte* during the past seventy-five years is obsessed by this idea. Or perhaps a better illustration would be to select from the enormous literature upon Christian Gnosticism such work as is represented by the volumes of Bousset,[1] Anz[2] and Amélineau,[3] who demonstrate respectively that the system sprang from Zoroastrianism, Babylonian astral worship and Coptic magic, each arguing exclusively from the philological antecedent of certain specific terms used by Gnostic writers. All of these scholars know and mention explicitly the religious syncretism which marked the countries and age in which Gnosticism arose,—that strange coexistence and intermingling of all sorts of exotic cults which people frequented in seeming indifference to their inconsistencies and contradictions. Yet, in spite of the established fact of this religious climate, none of the three authors I cite has hesitated to ascribe definite lines of direct derivation where the evidence justifies no further conclusion than the contemporaneous existence of these other religions. With them it is not, as it should be, a commonplace in their historical canon that the religious genius habitually seizes out of the air, as it were, a name or phrase or idea that is current in his day, and weaves it into his own system without the slightest consideration of its origin and proper significance, and often indeed without any further acquaintance with the system to which it originally belonged.

Where the evidence is scanty enough, the fallacy of spinning an elaborate theory of genetic relationship out of philological coincidences is not ridiculously evident; but an attempt to apply exactly the same method to isolated identities, where

[1] W. Bousset, *Hauptprobleme der Gnosis*, 1911.
[2] W. Anz, *Zur Frage nach dem Ursprung des Gnostizismus*, 1897.
[3] E. Amélineau, *Essai sur le gnosticisme égyptien*, 1887.

the general evidence is more complete, will usually lead to all sorts of obvious inconsistencies. For example, in the *Book of Mormon* there are phrases which imply the technical terminology used in a contemporary controversy over baptismal remission which divided the denomination known as Disciples.[4] If there were extant only the *Book of Mormon* itself and a contemporary anti-Christian polemic writing which contained, as quotations, scattered fragments from sermons by Disciple ministers, a future Bousset or Anz, if he were true to his method, would have to describe Mormonism as a subcult or offshoot of the "Disciple School."

For another example of failure to consider in historical deductions obvious mental peculiarities of the religious genius, one may look into the literature which has sprung up around the interpretation of the fact that Jesus spoke of himself as the "Son of Man." All sorts of far-fetched theories are drawn from this phrase in sermonic literature; but the opinions of scientific historians are scarcely less numerous and divergent. Aramaic and Syriac lexicography has been ransacked, arrays of texts are marshalled, and statistical tables compiled therefrom; but all on the tacit assumption that the term in question was deliberately chosen in the first place to represent a clearly defined and fully developed idea, and that it was used in exactly that sense and no other up to the end. So firmly established is this assumption that none of the writers upon the subject, so far as I know them, has given more than passing reference to Ezekiel's use of this term. As he used it, the phrase is simply a circumlocution for "a prophet;" Jesus, during the period reported in the Gospels, undoubtedly meant by the "Son of Man" one greater than the prophets; hence Ezekiel is ignored in favor of Daniel, who uses the term with messianic significance in words quoted by Jesus himself. Yet it is an outstanding fact in the biography of almost any religious genius that his terminology grows with his religious experience. Words that were used early in his career with simple literal significance are gradually filled with new meaning, until they become so highly technical that only a mind saturated with the whole doctrine may fully appreciate them. In the same way it is not a rare phenomenon to find a religious innovator whose system is directly influenced by the choice of his terminology. If in the teaching of other men he discovers connotations and definitions of certain words that he himself has used almost by

[4] Wm. A. Linn, *The Story of the Mormons*, 1902, 93 (footnote).

chance, he may unhesitatingly graft these new ideas into his own system.

If in the light of these psychological peculiarities we suppose that Jesus, early in his career, still looking upon himself as one of the religious leaders known as prophets, adopted Ezekiel's habit of designating himself as the "Son of Man;" and then, in the development of his messianic consciousness, brooding over the Daniel prediction, gradually added to the simple significance the more complex and technical connotation; we shall have a theory which will not only meet the facts of the case but which has the additional strength of support by psychological parallels in the history of other religious founders.

But nowhere perhaps in historical investigation is there greater need of a correct canon of psychological interpretation than in those problems which are concerned with pseudographic writings. To speak of these as forgeries and plagiarisms is to introduce ethical considerations which any study of the abnormalities of religious genius will show are entirely lacking. A greater part of Biblical criticism involves questions concerning documents of just this kind; and it would sometimes seem that the most formidable obstacle in the path towards a scientific solution of the problem is that complex of prejudices and repugnances which arises in most minds the moment it is suggested that a particular document is not from the hand of the author which it claims. The naïve procedure of a pupil who cuts out of his Bible every word and passage that any writer has declared "not authentic" is scarcely less scientific than the refusal of 'conservative' writers on Biblical criticism and patrology to consider evidence and arguments that they would accept unhesitatingly were the documents in question of secular instead of ecclesiastical origin.

There is, for example, the problem of the *Second Epistle to the Thessalonians,* which is still accepted by many modern scholars as of Pauline authorship, though perhaps the majority have come to deny its authenticity, on the grounds of its formal verbal identities with the *First Epistle,* and its explicit denial of doctrine propounded in that document. The general tone of the *First Epistle* is benignant, enthusiastic and impetuous; and it speaks of the second coming of the Lord as imminent, during the lifetime of the writer and those he addresses. The general tone of the *Second Epistle* is chiding, repressive and authoritative; and it teaches that before the second coming of the Lord certain events must occur, and presumably that

ages will first elapse. Furthermore, it denounces roundly those who refuse to perform their routine duties in life. Besides the fact that most of the introduction follows the *First Epistle* word for word, there is the significant statement that the letter is written lest "ye . . . be troubled, either by spirit, or by word, or by *epistle as from us*, as that the day of the Lord is just at hand . . ."

To reconstruct in the following terms the circumstances under which this second document was written, is not, it would seem, a gratuitous flight of the imagination, but as scientific a procedure by the laws of psychological analogies as it is for a palaeontologist to sketch an extinct animal from the evidence of some fossil bones. Certain early Christians were so impressed with the imminence of the Second Advent that they dropped the distractions of routine life and gave themselves entirely to contemplation and preparation for the catastrophe. In refutation of their fellows who disputed this theory, they exhibited as the words of Paul what we now have as the *First Epistle*. Among the anti-adventists who refused to be silenced by this argument was one who offered in refutation, not a rival interpretation of the passage, but a whole-hearted denial that Paul wrote the words at all; some evil-minded person had forged the whole epistle or had tampered with the text. Next, brooding over the matter, he undertook to rewrite the epistle as Paul probably had written it, copying exactly the insignificant introduction, but changing entirely the doctrinal portion, omitting the personal references that had only temporary significance, but warning explicitly against forgery as if it were foreseen, and railing at those who refused to work. With the revision once written, the next easy and final step would be the gradual conviction in the mind of the revisor that the document thus produced was wrought under divine inspiration or by the miraculous spiritual dictation of Paul himself. Later generations, ignorant of the local controversy of their fathers, would, of course, treasure both documents without distinction since each professed to be from the hand of the revered apostle.

II

These three examples of the ecclesiastical historian's direct application of methods and materials that are purely psychological are given, not so much for their intrinsic value, as in the hope that such a concrete presentation of sample problems may start some psychological reader upon investigations consciously directed towards the solution of other historical

questions. For there are numerous matters in which generalizations derived casually from readings in religious biography cannot meet the needs of the historian; in these he must await the scientific pronouncements of the professional psychologist. It may not, therefore, be amiss if, before sketching the main outlines of a few of these open questions, I attempt to describe the kind of solution that will best meet the needs of historical investigation.

In the first place, it must always be borne in mind that history is concerned with immediate, not ultimate psychical causation. However important the primitive origin of any given psychical manifestation may be in the eyes of pure science, it is only those phases which emerge from the mental surface that belong to the field of history. Psychoanalysis may be able to trace the religious ideas of an individual behind the veil of his consciousness, but the historian has no right or authority to penetrate that dim realm. Roughly speaking, it may be said that the historical record of any religious figure is complete when it has accurately described what he believed that he believed, how he presented his belief, and the way in which his followers understood that presentation; in the main, the course of religious history consists of little more than an overlapping series of these triple elements.

It is extremely unfortunate, so far as the study of history is concerned, that modern psychology is so largely analytical and so sparingly descriptive. In a science so young it is natural, of course, that the general problems should be worked and reworked many times before specific manifestations can be brought to the front; but the religious historian awaits somewhat impatiently the growth of an adequate mass of studies of specific religious types. Though it be not altogether just, he is inclined to complain that in general, when psychologists do treat of religious phenomena, they use such matters only to illustrate some broader psychological hypothesis, or that frequently, being interested in the main thesis, their selection of fact is not a happy one.

Similarly, in the eyes of the historian, it is unfortunate that in discussing religious experiences the psychologist so frequently uses imperfect written records rather than first-hand, modern sources. Looking at the field in a broad way, one can not but be struck by the fact that there is a situation which may be caricatured in these terms: one writer describes the psychology of Joanna Southcott by analogy to that of Joseph Smith, another justifies his estimate of Joseph Smith by the presence of similar mental peculiarities in Joanna Southcott.

But it is inevitable that such cross-arguments must continue until we have adequate scientific studies by competent psychologists upon contemporary religious founders. The religious genius is always with us; probably, if one were to count them over, he would find no decade since 1800 which has not seen the establishment in America of a new religion which won more converts during the first twenty-five years of its existence than did Christianity in its first two centuries. No generation lacks its believers in a gospel written yesterday, or even in a contemporary incarnation of divinity. The historian cannot express too emphatically his hope that the religious psychologist will leave to him the biographies of Joanna Southcott, and will go out with a note-book to some dingy hall where a new gospel is preached. Nor can he repress a sigh of regret when he notices that James entered on his career in the Philosophical Department of Harvard one year before Mrs. Eddy opened her "Massachusetts Metaphysical College" in Boston, and then remembers how many of the "varieties" James chose to describe were drawn from the pages of eighteenth century Quaker biographies. Indeed, the same historian must not be judged too harshly if he sometimes declares that James' work, as a psychological treatise, is inferior to Howells' "Leatherwood God," despite the admixture of fiction in the latter.

III

The problems themselves that demand solution are obvious. Division of the individuals to be described into three classes according to their respective functions in the course of a religious movement will show (*a*) the religious genius, (*b*) his personal disciples, and (*c*) ordinary adherents to the system. The groups produced by this functional classification seem to show distinct types of temperament, which deserve separate study.

Of the first of these little more need here be said. The generalizations that the historian has made concerning the psychological abnormalities of the religious genius need verification and restatement; and undoubtedly there remain more traits and habits of mind common to this type which will be discovered only by careful comparative study of living subjects. It has already been pointed out that material for such first-hand investigation is usually available.

In the examples cited in the first part of this paper we have not mentioned the second group, the personal disciples of a founder. These disciples seem to have played an important

part in the development of every religious movement. Though they lack the leader's originality, their influence may have been more or less decisive both by their polarization of his teachings as it passed through them to others, and by the reflex action of their understanding of his message upon the master's own final formulation of it. Usually historical sources preserve but little record of these disciples beyond their names, yet now and again one catches hints of the characteristics common to this type,—abnormal willingness to believe, fiery enthusiasm, unbounded liberality to "The Cause," and a lack of candor, often approaching deliberate trickiness, but without that common sense in practical values that sometimes stands out so amazingly in the genius himself.

In the third class the characteristics of the type will usually be found in the highest development in the official clergy; but any study of these professional religionists must be corrected by a corresponding examination of representative laymen, that due allowance may be made for the elements that belong, not to the religious experience, but to the office. That the different religions present specific types and sub-types must be the conviction of every curious observer; whether or not these types are persistent and amenable to classification is another question. Certainly the value of such classification, should it prove practicable, would be so great that one may hesitate to pass over as valueless even so frivolous an observation as Mozley's remark concerning "evangelical sallowness;" "How is it that goodness, poverty, and a certain amount of literary and religious ambition produce an unpleasant effect on the skin?"[5]

From the historian's side these characteristics, whether they be cause or effect, are of supreme importance; if certain temperaments are attracted by corresponding climates in religion there will follow, in the course of years, a reflex action upon the religion itself, tending toward more exact conformity with the religious taste of that temperament. If the existence of such a reaction can be definitely established, it will do much towards explaining on rational grounds the frequent recurrence in religious history of particular phenomenal series. Some writers are inclined to explain these on genealogical lines, bridging any chronological gap with a bold hypothesis of direct influence, while others, with less success, seek to explain them by laws drawn from a philosophy of history devised to meet

[5] T. Mozley, *Reminiscences chiefly of Oriel College and the Oxford Movement*, 2nd ed., i., 1882, 243.

the occasion; still other writers are content to remark that religious history persistently "repeats itself."

In the same way it is probable that the development of descriptive religious psychology will produce much material of importance in the solution of analytical problems. For example, those disquieting figures that appear in every religious body, who admit in private without scruple that they disbelieve in the possibility of any religion at all, and in public are the most zealous observers and advocates of their official creed down to its smallest denominational peculiarities, will probably be found to represent a distinct type of religious experience. Because these men appear most frequently in ritualistic circles, it is often said of them that "their religion is purely aesthetic." If this phrase, which sounds like an explanation, be true, it will be a fact of enormous significance in any attempt to define the religious sense. If on the other hand, as seems more probable, the religious experience of such men is really religious despite the denials of their materialistic metaphysics, that likewise will be a fact of far reaching implications.

Finally, it will be noticed that not until there has been accumulated an enormous bulk of descriptive investigations will it be possible to define the religious experience of normal human nature. Religion necessarily offers many attractions to the abnormal human mind; the high proportion of freaks in any group of religious has persuaded not only many laymen but some scientific men as well that all religious experience is fundamentally pathogenic. Whether this be true or not is, however, a matter of no great significance either for the historian or the psychologist; to both "religion is an incident in human history and a manifestation of human nature," and as such will remain an object for scientific study.

DEATH-PSYCHOLOGY OF HISTORICAL PERSONAGES

By Arthur MacDonald, Washington, D. C.

A summary of the last words of those distinguished people in history, records of whom have come down to us, is a psychology of their death.

Before presenting the results of such a summary, and in order to understand better the significance of words during the dying hour, it may be useful to note a few points as to death itself.

The Dying Hour

In another place, the author has treated in detail the physiology and psychology of death.[1] Here it may be stated that death is neither rapid nor sudden, but is preceded by a period of transition, which begins as soon as the reactionary forces of the organism have ceased and combat has ended.

The death-act is often confounded with the symptoms of disease which precede it. Dying begins after these symptoms have subsided; there is a pause in nature, the disease has conquered, the battle is over, and all is tranquil.

This transition-stage, or dying hour, may last for a longer or shorter time; in the great majority of cases persons are unconscious. Thus the natural death appears to be a brain-death.

But when there is consciousness during the dying hour, it depends upon nutrition and provision of the brain with blood. As there are three ways of physical death, (1) by brain, (2) by heart, and (3) by lungs, so there are three kinds of psychological death. The first is where there is little or no delirium, and intelligence not only continues to the end, but becomes very acute; physical prostration appears to be replaced by intellectual exaltation. Another kind of psychological death results from diseases only secondarily connected with the brain; the mind is in a mixed state between reason and delirium. The third kind of mental death includes all the lesions of the brain, which are almost always accompanied by loss of understanding; delirium is a symptom; there is a general obscuration of intelligence, and complete loss of consciousness.

[1] *Medical Times*, N. Y. City, 1921.

Fear of Death

In life the fear of anything is often much worse than the thing itself. This is especially true in the case of death. When the dying hour comes, the fear of death disappears.

Whether it is the brain, heart or lungs which give the signal of death, the brain-forces are usually weakened or destroyed first, causing sensation to lessen or cease. Whether there be consciousness to the last, or only at times, depends upon the nature of the disease and the mental and moral character of the person dying; and this in connection with surrounding conditions. In old age, death is the last sleep, showing no difference from normal sleep. The general consensus of opinion based upon the experience of all ages is that the dreadfulness of death and its physical pain are for the most part in the imagination.

Psychological Summary of Death of Distinguished Persons in History

The average man usually dies unknown; whatever he thinks and says is soon forgotten. Fine and significant words may be lost. Now and then a physician may take the pains to note the last words of some of his patients. There may be a dozen cases, or more; but the outcome is small compared with the great number of those dying every day. Therefore almost all the last words recorded at death are those of eminent and distinguished persons.

This table represents a first attempt to summarize the mental condition, at or just before death, of distinguished persons from the beginning of history up to the present time. Only the most reliable sources have been utilized; and even here where there appeared to be any doubt the persons were omitted, so that we have remaining but 794 cases. It must be remembered, however, that very few death-bed experiences are published, and still fewer are described with sufficient accuracy to be made the object of scientific study.

Taking into consideration the very many and varied sources, in all periods of history, which have been consulted, the regularity of the figures in the table is remarkable. Such uniformity, coming out of most heterogeneous conditions, when put into statistical form, suggests that death is a great equalizer and leveler for all humanity. In a way, the conduct and last words of those facing death are a mental and moral test of their real character.

PSYCHOLOGY OF DEATH OF DISTINGUISHED PERSONS IN HISTORY

Profession or Occupation	Number of Persons	Average Age	Manner of Death: Violence: Executed	Killed in Battle	Suicide	Assassinated	Total	Disease	At Death Number: Little or no Pain	Pain	Mental State of Dying Persons: Last Words At or Just Before Death: Number of Persons: Religious	Sarcastic, Jocose	Request, Directions Admonition	Question, Answer, Exclamation	Contented	Discontented	More or Less Indifferent	Average Number of Words: Religious	Sarcastic, Jocose	Request, Directions Admonition	Question, Answer, Exclamation	Contented	Discontented	More or Less Indifferent
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
Religious	192	56	42	0	0	4	46	146	12	3	39	6	26	10	66	17	30	23	10	17	2	25	22	19
Royalty	91	50	23	5	2	7	37	54	5	4	11	7	8	11	13	15	16	18	17	23	6	20	23	12
Military	75	51	21	21	1	6	49	26	31	2	2	1	20	8	15	13	17	11	0	16	6	11	16	17
Philosophers	102	60	7	0	5	2	14	58	5	0	6	6	11	14	28	8	17	9	16	11	5	8	11	20
Literateurs	106	64	6	2	1	2	11	95	10	0	9	24	8	9	23	10	27	18	12	14	5	14	15	16
Physicians and Scientists	54	64	3	0	0	0	3	51	4	2	4	1	8	3	18	3	10	9	0	8	5	23	11	10
Artists	39	63	0	0	0	0	0	39	4	2	3	2	0	8	7	4	9	21	13	0	5	12	16	9
Poets	69	56	3	1	1	0	5	64	3	2	4	5	8	8	17	2	18	20	24	14	3	15	16	15
Statesmen	96	65	18	1	2	6	27	69	6	4	8	5	14	10	26	14	17	14	45	18	3	10	14	12
Women	70	56	29	0	3	0	22	48	7	3	7	3	9	6	17	11	9	12	34	63	8	11	15	15
Totals Total Averages	794	60	142	30	15	27	214	580	59	21	93	60	112	87	227	96	170	18	20	19	5	16	16	14
Percentages			18	4	2	3	27	73	63	35	26	17	31	26	46	19	35	Average Variations						
																		13	11	16	3	9	9	9

The persons whose records we have studied are classified, according to occupation, into ten divisions, as indicated in the first column of the table. Those whose profession was of a religious character are the largest in number (192), which is due doubtless to the power of religion throughout all history. Under "philosophers" are included mathematicians and educators. As the number of women was not large enough to make subdivisions, all women are placed together.

It will be seen from the third column of the table, which gives the average age, that the great majority of men who become eminent must live at least fifty years. Royalty and Military show the lowest average age, due in part to the large number of deaths by violence, which is the case also with Religious, Statesmen and Women. In short, all the ages in divisions where there are many deaths by violence would of course have a much higher average age had they lived their natural lives.

If we eliminate this factor of death by violence, the Poets and Artists die the youngest; thus Keats died at 26, Byron at 36, Burns at 37, Poe at 38, and Addison at 47. In the columns for pain or little or no pain at death, it will be seen that in only 80 cases out of 794 was any reference made to this matter, indicating that the question of pain at death is regarded as of little importance. In the last 14 columns of the table is presented the mental state at death or just before death, as shown by the last words. It will be noted, from column 13 at the bottom, that 17 percent were sarcastic or jocose, indicating a high degree of mental control. In fact some of the dying complained that it was taking too long and they were getting tired. A relatively large number (24, or 37 percent.) of writers and authors (literateurs) were jocose or sarcastic or both (column 13); they also were relatively the freest from pain (column 10).

The Military show much the relatively highest number of requests, directions or admonitions (column 14) in their last words. The Philosophers stand relatively high in questions, answers and exclamations (column 15). In general it will be noted (columns 12-15) that requests, directions and admonitions were most frequent (31 percent).

More than twice as many (46 percent.) were contented than were discontented (19 percent), as is seen at the end of columns 16 and 17; this accords with the fact that 65 percent had little or no pain, and 35 percent had pain. Thirty five percent were indifferent (column 18); but they all took about the same number of words to express their feelings

(averages, columns 23, 24, 25). While relatively few of the Statesmen and Women were sarcastic (column 13), they took many more words to express themselves (column 20) than the others; the Poets also had as high an average as 24 words. In requests, directions and admonitions the Women show an average of 63 words, which is three times as great as that of any of the others, except Royalty, which has 23 (column 21).

As to expressing contentment or discontentment, the Religious and Royalty used the most words, except for contentment, where the Physicians and Scientists have an average of 22 words (column 23). The Artists and Scientists used the fewest words of all (averages 9, 10) to express their indifference (column 24).

References

Book of Death.
Chambers' Biographical Dictionary.
Egbert, Walter R. Last Words of Famous Men and Women. Morristown, Pa., 1898, 792 pp.
Kaines, Joseph. Last Words of Eminent Persons. London, 1866, 397 pp.
Lippincott, Biographical History.
Marvin, Frederick R. The Last Words of Distinguished Men and Women. New York, 1901, 80, 336 pp.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

Communicated by JOHN E. ANDERSON

I. AN EXPERIMENT IN TIME ESTIMATION USING DIFFERENT INTERPOLATIONS

By LLEWELLYN T. SPENCER

The problem of the perception of time has been given frequent and almost continuous treatment. Much has been done, especially in Germany, in regard to difference limina, increased uncertainty with increased length of interval, etc. In this country, among other investigators, R. McDougall[1] with thirty subjects found sex differences. He found that women are less accurate than men in their estimates of time-intervals and had a greater tendency to overestimate. Yerkes and Urban[2] conducted a group experiment on a far larger scale, in order to study this difference. Four intervals were used, 18, 36, 72, and 108 sec. The subject recorded his estimation of the interval in seconds. The interval was given by the signals "start" and "stop" spoken by the experimenter. With each of the intervals four different "fillings" or interpolations were used. These were idleness, reading (by the experimenter), writing (at dictation), and estimation (any method except the use of a timepiece). In the experiment approximately two hundred and fifty males and approximately two hundred and fifty females were used. This made a grand total of over 8,400 observations. Of all the observations for all the intervals and interpolations 2.39% were correct for males and 1.05% for females. This bore out McDougall's findings. They also found that the number of correct judgments decreases with the increase in the length of the interval. But upon examining their results, they discovered that in the judgments reported the final digit was most often a multiple of five; 65.9% of the male judgments and 80% of the female judgments ended in 5 or 0. This frequency is too great to be due merely to chance. They also found that even numbers were more frequent than odd numbers as the final digit. The order of decreasing frequency for the last digit was 0, 5, 8, 2, etc. In other words judgments "were strongly influenced by the thought of the conventional time unit, the minute, for in all quantitative work there are errors in favor of the standard of measurement and simple fractions thereof. . . . The longer an interval . . . the more frequently it is judged as the same as the unit or a simple fraction of that unit."[3] Inasmuch as the intervals used were not multiples of 5, it is clear that a certain influence not present in normal behavior affected the judgments of the subjects in the investigation of Yerkes and Urban. The present investigation was undertaken to determine whether a method of judgment which is not

[1] *Science*, N. S. 19, 1904, 708-709.
[2] Harvard Psychological Studies, 2, 1906, 405-430.
[3] *Op. cit.*, 417 and 418.

subject to this influence would produce results varying significantly from those obtained by Yerkes and Urban. The method chosen was that of reproduction.

The interval was given by a sharp rap on the table with a pencil or ruler as a signal of the beginning and of the end of the interval. The subject immediately reproduced the interval by starting and stopping a stop-watch, which was enclosed in a felt-lined case to prevent any reference to the rhythmical ticking. In this way the judgment was freed of any prejudice from "the thought of the conventional time unit." The judgments could be obtained to the fifth of a second. Four intervals were used, 15, 30, 60, and 100 sec. These closely approximate those used by Yerkes and Urban. The experiment was divided into two parts. In the first part the four intervals were given in mixed order, without any interpolation. This corresponds to the "filling" called "estimation" by Yerkes and Urban and will hereafter be designated by that name. The subjects were instructed to reproduce the interval immediately after the final rap, without using any

TABLES OF RESULTS

TABLE I.
15 SEC.

Estimation

Sec.	Cases
10–14	12
15–19	43
20–24	17
25–29	11
30–34	5
35–39	4
40–44	3
45–49	1
50–54	1
55–59	2
60–64	1

TABLE II.
30 SEC.

Estimation

Sec.	Cases
20–24	9
25–29	18
30–34	22
35–39	17
40–44	8
45–49	11
50–54	5
55–59	4
60–64	2
65–69	1
70–74	1
75–79	1
.....	..
105–109	1

TABLE III.
60 SEC.

Estimation

Sec.	Cases
30–34	1
35–39	2
40–44	2
45–49	7
50–54	9
55–59	27
60–64	11
65–69	7
70–74	7
75–79	6
80–84	7
85–89	1
90–94	1
95–99	4
100–104	2
105–109	1
110–114	1
115–119	1
120–124	1
.....	..
150–154	1
165–169	1

TABLE IV.
100 SEC.

Estimation

Sec.	Cases
35–39	1
40–44	..
45–49	1
50–54	2
55–59	1
60–64	3
65–69	1
70–74	4
75–79	2
80–84	9
85–89	7
90–94	7
95–99	11
100–104	7
105–109	8
110–114	8
115–119	7
120–124	2
125–129	4
130–134	1
135–139	2
140–144	2
145–149	3
150–154	1
155–159	2
160–164	1
170–174	2
200–204	1

method of rhythmic comparison; that is, counting in the stimulus and response intervals, humming tunes, or thinking poetry, and thus gaining a basis for comparison, were forbidden. During the reproduction the subject was idle both in the first set of observations and in the second set, described below. Ten subjects were used throughout the experiment. These were men of the undergraduate body of Yale College and the Sheffield Scientific School and of the Yale Psychological Department. The four intervals were given twice at each sitting and only one sitting was held each day. No attempt was made to keep the sittings regular, but each subject had five sittings on each interval or interpolation, which makes a total of one hundred judgments on each interval or interpolation. As the subjects were ignorant of the success of their judgments, practice-effects could have small influence. Tables I, II, III, and IV give the distribution of judgments for these four intervals in classes of five.

In the second part of the experiment, the 30 sec. interval was used with the following interpolations: Reading of prose by the experimenter, Reading of prose by the subject, Reading of poetry by the experimenter, Reading of poetry by the subject, and Dictation. The interpolation was of course used only in the stimulus interval, and during the reproduction interval the subject spent the time in idleness as in the period of estimation described above. For the prose, selections from current periodicals were used and familiar and especially rhythmical poems were employed in the intervals devoted to poetry. Dictation was from the material of the reading. Table V gives the distribution of judgments for these five interpolations. For convenience of reference, the 30 sec. interval with estimation (idleness) is included also in this table.

TABLE V

INTERPOLATIONS FOR 30 SECS.

Judgment	Estimation	Prose By Experimntr	Prose by Subject	Poetry By Experimntr	Poetry by Subject	Dictation
5–9						2
10–14			1	1		10
15–19		5	1		2	9
20–24	9	4	6	9	8	10
25–29	18	18	15	5	10	14
30–34	22	24	16	22	16	18
35–39	17	11	19	18	15	8
40–44	8	14	12	24	14	9
45–49	11	9	7	7	13	11
50–54	5	7	11	6	7	3
55–59	4	6	6	3	7	2
60–64	2		2		2	
65–69	1	1	1	3	1	1
70–74	1		2	1	2	2
75–79	1					
80–84			1		1	
85–89					1	1
90–94					1	
95–99		1				
100–104				1		
105–109	1					

Of the nine hundred observations, thirty-five are correct. Responses were read to the nearest second, under the assumption that responses within two-and-one-half fifths either side of the unit second were equally distributed within those limits. This gives a percentage of 3.88% correct. Yerkes and Urban found the percentage to be 2.39%, as stated above.

The effect upon the percentage of correct responses of the prejudice in favor of the multiples of 5 can be approximately calculated. It is evident that this prejudice increases the returns for two of the possible final digits, at the expense of the other eight. It is assumed that this prejudice operates in a fairly regular manner. Such an assumption is always made in the description of any psychological trait. For example, we assume that every time the Müller-Lyer illusion is presented there will be a constant tendency to misjudge the distances. The percentage given by Yerkes and Urban of 65.9% of the responses ending in 5 or 0 represents the expected returns for those digits *increased* by a certain percentage at the expense of the other digits. Of all the responses only 20% should end in 5 or 0, on the basis of pure chance. Hence the difference between the 65.9% obtained and the 20% expected, viz., 45.9%, represents an increase of the returns for 5 or 0, due to the disturbing influence. This percentage should be redistributed among the other eight digits, so that 45.9% ÷ 8 = 5.73% approximately should be allotted to the percentage of cases of the other final digits. I say 'approximately,' for it was shown in their article that the other final digits were also affected by the prejudice, but not by any means to such a great degree. But this addendum of 5.73% refers only to the final digit in each case. We are interested in the correct tens digit as well. Only a portion of this addendum should be allotted to the particular percentage of correct final digits falling within the correct tens. There are obviously cases where the final digit is correct but the tens digit is incorrect. An examination of the data given in the article of Yerkes and Urban shows that 24.86% of the responses were of the correct tens digit. Therefore 24.86% of the addendum should be added to the percentage of cases in which a correct response occurred; and 24.86% of 5.73% is 1.41%. This percentage, then, should be added to the percentage obtained while the prejudice in favor of 5 or 0 was operative. The percentage of correct responses obtained by Yerkes and Urban was 2.39%; and 2.39% + 1.41% = 3.80%. This should give us the correct percentage of correct responses obtained when the prejudice referred to is not present. As a matter of fact, in the present experiment, the percentage of correct responses found was 3.88%. The difference of only .08% between the theoretically calculated percentage and the percentage actually obtained is so small that we are justified in considering that the amounts agree in a striking manner.

It may be objected that the present argument assumes that the judgments were distributed purely by chance, whereas the distribution clearly shows a tendency to judge the interval approximately correctly. It is certainly true that the argument rests upon that assumption. But such an assumption is limited to the distribution *within the tens*. In other words, we assume that, while the ability to judge time-intervals is sufficient to enable us to approximate the correct length, this approximation is only to the correct tens digit and not as close as the final digit. Within the tens the distribution of the responses is assumed to be fairly in accordance with the laws of chance. It is possible to discover from our data whether this is actually the case. An

examination of the results gives the following distribution of responses in terms of the final digits within the correct tens.

Distance of digit from correct digit	—4	—3	—2	—1	0	+1	+2	+3	+4	+5
Number of cases	20	25	30	20	35	41	25	30	38	30

This shows that the distribution is approximately by chance and is by no means grouped about the correct digit. It is therefore justifiable to assume the chance distribution within the correct tens, which is required by the preceding argument. If the calculations are correct, this close correspondence between the theoretically calculated percentage and the percentage actually observed confirms the accuracy of the present experiment, and shows conclusively that the prejudice mentioned did decidedly affect the judgments of the subjects of Yerkes and Urban's experiment. The difference in the percentage of correct responses as reported by the two investigations is therefore clearly traceable to the difference in method.

Using the mean as a measure of central tendency, Yerkes and Urban found that the 18 sec. interval of "estimation" was overestimated (by 1.5 sec.). In the present investigation the 15 sec. interval in estimation is overestimated also (by 7.27 sec.). In this the two experiments agree. For the longer intervals there is a marked difference. For the 36, 72, and 108 sec. intervals of estimation they found an underestimation of 2.9, 8.8, and 0.2 sec. respectively. I find an *overestimation* in the case of the 30, 60, and 100 sec. intervals of 8.23, 8.55, and 4.31 sec. This difference in the direction of error may be a significant function of the methods used. Table VI gives the means mentioned, as well as the medians and measures of variability. All were calculated from the unclassed measures.

TABLE VI

Int. in Sec.	Interp.	Mean	σ	PEm	Mdn
15	Estimation	22.27	7.79	.52	19.5
60	Estimation	68.55	21.40	1.44	61.1
100	Estimation	104.31	26.35	1.77	100.5
30	Estimation	38.23	12.20	.82	35.2
30	Prose by Exp.	37.25	12.05	.81	34.8
30	Prose by Subj.	38.36	10.89	.73	36.6
30	Poetry by Exp.	39.07	12.25	.82	37.3
30	Poetry by Subj.	40.89	14.21	.85	38.7
30	Dictation	32.41	10.65	.67	30.3

In the course of the experiment it was noticed that a subject would occasionally lose all track of the interval, or would be disturbed by some thought or unavoidable distraction, so that certain responses were without doubt abnormally shortened or prolonged. The judgment of 105 sec. for a 30 sec. interval of estimation is certainly not free from some uncontrolled influence. In consideration of this fact, it seems that a measure, such as the median, which does not consider

the value of the extremes, is a more accurate measure of the central tendency than the mean. Using the median as a measure, the 15 and 30 sec. intervals are seen to be overestimated. The 60 and 100 sec. intervals show no definite overestimation if the median is used.

In summing up, the following points are significant:

1. The method of reproduction produces more accurate results than the method of statement in terms of the standard unit. The error of the latter method in the case of Yerkes and Urban's results was theoretically calculated, and the corrected percentage of correct responses is shown to agree with the percentage obtained by the former method.
2. While the percentage of correct responses is greater when the method of reproduction is used, the percentage thus obtained (3.88%) is very low and shows that time-estimation is not very accurate.
3. Results obtained by the method of reproduction do not agree with those obtained by the method of statement in terms of the standard unit in showing that all intervals of greater length than 18 sec. are underestimated. On the contrary, all intervals were overestimated when the method of reproduction was used. If the median is used as the measure of central tendency, the overestimation of the 60 and 100 sec. intervals is not significant. This is true no matter what interpolations are used.
4. Poetry as an interpolation causes the interval to be estimated as longer than an interval with no interpolation (*i. e.*, estimation) or than one with the reading of prose.
5. The interval is estimated as longer if the subject reads the prose or the poetry than if the material is read by the experimenter.
6. Dictation causes the interval to be estimated as shorter than one with any other of the interpolations used, but not necessarily shorter than the actual interval.
7. The results of Yerkes and Urban agree with this study in the comparison of one interpolation with another, as to the effect upon the judgments, although they vary from the present findings in the relation of the judgments of the intervals to the actual intervals.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

LIII. The Involuntary Response to Pleasantness

By G. H. Corwin

In a recent paper on *Pleasantness and Unpleasantness in Relation to Organic Response,* Young says:[1] "While U is associated with a large variety of reflex movements, P is passive and negative. Our data do not contain a single case of active reflex response to a P stimulus. With U there are withdrawing movements, frowning, straining, reflexes of expulsion, etc., while P is characterized by mere acceptance of the situation and the passive yielding to it. . . . Muscular strain (tension) is correlated with U, while its opposite, relaxation, is correlated with P.

"The traditional relation between P and seeking movements finds little support in our reports, while that between U and withdrawal is abundantly confirmed. The entire evidence for seeking movements, as 'expressions' of P, is found in 11 reports, in which the subject "sniffed," "took a deep breath in order to get more," "held the breath," etc. In every case these seeking movements are deliberate and never reflex, like most of the withdrawing movements of U It is a question to what extent and in what sense voluntary deliberate behavior based upon the knowledge that P may be produced or prolonged, or U avoided, can be considered an 'expression of feeling.'"

This result, that the typical involuntary reaction to P is a relaxation and not a seeking movement, is contrary, as Young says, to psychological tradition; and it occurred to us that it might be occasioned by the method which Young employed. In fact, he himself implies that, under other conditions, another result might have been found. "It should be remembered that any result is a function of conditions. Our subjects were seated quietly in a Morris chair, instructed to be 'passive and receptive' and 'to let the experimental situation have its full normal effect.'" We have therefore undertaken in our experiment to put the *O* in a different situation. Instead of giving him a stimulus under conditions in which a seeking movement would be not only unnecessary but also impossible, and further instructing him to be 'passive and receptive,' we have tried to create a situation in which the *O* must seek if he desires to retain a P, and also to find an instruction under which he might move without disobeying it.

Our experiments fall into three series according to the stimuli used. We shall designate them as the olfactory, cutaneous and auditory experiments.

The Olfactory Experiment. The *O*'s were seated in an ordinary chair in a room especially prepared for the experiment. To the right of the *O* a Ludwig kymograph was placed upon a table. This was concealed by a grey screen. The revolving drum had been removed, and a wooden rod 58 by 2.8 cm. was screwed to the center of the revolving disk. A clamp was fastened to the free end of the rod in

[1] P. T. Young, this Journal, xxxii, 1921, 38ff.

such fashion that the stimulus-vials could be easily inserted and removed. By means of this apparatus a vial could be placed on the end of the rod directly under *O*'s nostrils; when the kymograph was set in motion, the stimulus would recede from him at the rate of 1.7 cm. per sec. A small electric fan, which served the double function of keeping the air in motion and of eliminating the noise of the kymograph, was operated during this experiment.

As a check upon *O*'s involuntary movements we took graphic records throughout this series. For this purpose a band was placed around *O*'s head and a cord extended to an ergograph which carried a pointer. By this arrangement any backward movement of the head was indicated upon the drum by a line in an upward, and any forward movement by a line in a downward direction.

The *O*'s were Assistant Professor L. B. Hoisington (H), Dr. H. G. Bishop (Bi), and Miss D. B. Dewey (D), a student taking her 'major' in psychology. Despite the fact that H and Bi worked with some knowledge of the problem, H assures us that his results are not influenced by knowledge, and that his reactions were entirely involuntary; whenever Bi suspected that knowledge might have influenced his results, he reported the fact. Every *O* worked two or three hours a week for periods of 40-60 min. The work was done during the spring of 1921. Five other (untrained) *O*'s were occasionally introduced into the experiment as a control. In every case we found positive results similar to, although not as extensive as, those reported by H, Bi, and D.

A preliminary series extending over two weeks was completed in order to determine the most effective stimuli and the best form of instruction. The following stimuli were employed: vanillin and cinnamon (3 to 1); Cashmere bouquet toilet water; whiskey; ol. rutae; carbon bisulphide; asafoetida; and empty vials. The instructions were: "You will be given a series of olfactory stimuli, some of which are *intensive* odors, some have no odors at all. At the signal 'now' smell the stimulus. You are asked (1) to report whether the immediate experience is pleasant, unpleasant or indifferent, and to indicate the *intensity of the feeling* (using, for example, such terms as 'very weak,' 'weak,' 'moderate,' 'strong,' 'very strong;') and (2) to report all muscular tendencies and organic sensations in any way related to the affective reaction."

From our preliminary series it had become evident that the very instruction 'to smell' might predispose the *O* to continue smelling, and might therefore occasion a pursuit for another reason than P. To offset this difficulty and also other predispositions, such as for example, curiosity, interest, the "laboratory attitude," etc., we introduced as stimuli several empty vials, and informed the *O* in the instruction that some of the stimuli would be *intensive* odors and some would have no odor at all. It turned out, however, that whenever an *O* actually moved or had a tendency to move from any of these extraneous motives, the fact was always reported in his introspections.

We give in Table I a summary of our results in quantitative form. It will be observed that for all *O*'s pursuit movements occurred in about one-half of the cases in which P was reported. Furthermore, for H and D, in addition to definite pursuit tendencies or movements, there was a much larger proportion of characteristic reactions which indicated their desire to prolong or 'get more of' the stimulus. H

reports organic stir with P in 17 cases; it is almost never found in the reports of Bi and D. These *O*'s, however, were in 9 and 17 cases respectively actually seen by *E* to make gross pursuit movements.

TABLE I

Observer	H	Bi	D
Total No. Reports	73	46	60
Total No. Reports classified as P	36	26	42
No. of P reports characterized by definite seeking movements or tendencies to movement	15	13	18
No. of P reports characterized by secondary seeking movements, such as: sniffing, raising shoulders, change in breathing, extending arms, etc	34	11	28
No. of P reports with organic stir	17	0	5
No. of P reports with gross observed movement	0	9	17

Examples of the kind of introspection that we have classified in line 4 of the table (definite seeking movements) are:

D. "I felt attracted toward it and then repelled as it [the stimulus] became U." "Pushing of the shoulders and head forward." "Straining of the trunk upward." "Relaxing of my trunk towards the smell." "Moderately P, and I was inclined to follow it." "Tendency to go toward the stimulus not in a relaxed way but in a tense impulse." "I felt a tendency to move toward the stimulus."

Bi. "I felt a general pressure-kinaesthesis that would favor following up the odor, and when the stimulus moved away I felt also a decided pressure in my thigh-muscles Knowing what I do about the experiment, I am very careful not to read any movement into it. This definite movement of pursuit, however, quite surprised me; it was just as sharply automatic as the movement which one makes when his hat blows off. When the stimulus suddenly disappeared I suddenly felt myself in pursuit." "That's asafoetida! I liked it at first, and then it became U. Both were weak. I seemed to follow it at first and then, when it became U, I stopped." [He followed it and then jerked back.] "I felt muscular sensation in my head as I followed." "I felt like staying with that, keeping right after it."

H. "A general tendency to strain forward which was different from the other sort of expansive leaning." "Tendency to lean." "Tendency to react forward." "As it weakened there was a tendency to try to get more of the odor and there was a tendency to lean over. I must have leaned." "There seemed to be a bodily set of getting the odor; of going after the odor." "Thought I noticed an impulsion to get more of the experience."

Examples of line 5 (secondary movements) are:

D. "Relaxation of the head." "Relaxation of the neck." "Increase in saliva." "My shoulders raised with the inhalation." "Feit my trunk relax toward the stimulus." "Took long breaths to get all the sensation possible." "Tendency to take quick breaths." "Tendency to sit up towards the stimulus."

Bi. "Certain tendencies to keep on sniffing." "Just an easy relaxed kind of P." "One continuous uniform inhalation." "Just a free comfortable inhalation." "Pleasant relaxation all through the body. I had the relaxed, unrestrained breathing that comes with P." "Whole lot of sniffing movements. Kinaesthesis in the throat. Free nostrils." "A feeling in my face, as when I am just about to smile. These facial reactions were directly related to the P." "Bodily relaxation and comfortableness."

H. "A rather expansive tendency, that is, to throw the diaphragm down." "Slight tendency to deeper inhalation." "That same tendency to deeper, prolonged inhalation and rather more rapid expiration." "Certainly a muscular relaxing effect, very different from the experience one gets with U." "Seemed to be a tendency to get more of the odor." "Deep breathing. This sets up certain kinds of pressure in the abdominal region; not very strong in intensity. They have a lively something about them." "There seemed to be a tendency to a general muscular, I was going to say, relaxation, yet not in the sense of going flabby. The tonus of the muscles seems to stay rather high and yet not to constrict." "A peculiar muscular tendency in the direction of relaxation but with a fair degree of tonus." "There is an expansive effect different from relaxation, for there is still a high degree of muscular tonus, the muscles do not go flabby, there is a sort of brightness, a kind of glow to it." "There was muscular relaxation; I don't like that word 'relaxation' because the experience is as much a contraction as it is relaxation; perhaps a different set of muscles is involved. It may be characterized as 'expansive;' a 'glow.'" "There was certainly a muscular tendency both to relaxation and expansion (not going flabby). There is apparently a qualitative difference between the pressures when muscles contract and when they relax. There seemed to be an urge to get more of the stimulus." [*E* asked H to explain his statement about the muscles. H replied that contraction was correlated with U, it had a dull, uniformly intense nature. The experience was more or less definitely localized and definitely limited. Extension was correlated with P, and had a bright quality. It was more generally diffuse, was very indefinitely limited, and its intensity fluctuated within narrow limits.]

Examples of line 6 (organic stir) are:

D. "Made me feel hungry, empty feeling in my stomach." "It made pressure on the wall of the stomach." "I felt contraction of the stomach."

H. "There was a weak organic sensation in the region of the stomach, still there, diffuse, not definitely localized." "A bit of fleeting organic." "Slight pressure in the abdominal region." "Something like organic that eludes me all the time. Rather a bright-like pressure." "Weak organic something in stomach region, very indefinitely localized."

The Cutaneous Experiment. This experiment was conducted in the same manner as the Olfactory Experiment except that the first sentence of the instruction was made appropriate to the change in stimuli; in place of odors, five cutaneous stimuli were used. These stimuli were a draught of cool air, cold iron, a board containing fine sharp points, a board covered with sand-paper, and a board covered with soft fur. These were moved slowly across *O*'s forehead or nose.

It will be noted from Table II that, although the series is short, H found decided movement or tendencies to movement with *every* P

experience; also that Bi again reported no organic stir with P. It will also be seen that what we have classified as secondary characteristics of P were less marked and primary characteristics were more marked in this than in the olfactory experiment. There was only one case reported of a change in respiration.

TABLE II

Observer	H	Bi	D
Total No. of Reports	16	13	10
Total No. of Reports classified as P	9	8	5
No. of P reports characterized by definite seeking movements or tendencies to movement	9	4	4
No. of P reports characterized by secondary seeking movements, such as expanding, change in breathing, changing from nose to whole cheek, etc	6	1	2
No. of P reports with organic stir	5	0	2
No. of P reports with gross observed movement	4	3	3

Examples of the kind of introspection that we have classified in line 4 of the table are:

D. "Tendency to follow the stimulus in order to keep it next to the neck."

Bi. "I did have a tendency to put my cheek on it and follow it." "A little follow-up tendency."

H. "Then there was a positive muscular tendency forward." "There was a tendency to lean forward a bit so that the sensation would be less of a ticklish sort, but a rubbing with a fair degree of pressure." "I did not realize how P it was until it was over. When the support was removed I toppled over." "Same tendency to lean toward the stimulus and to maintain movement." "The stimulus is more at your command here. It is there, you know it is there, and you can do with it what you want. Quite a low level of attention anyhow."

Examples of line 5 are:

D. "Organism seems to expand and to grow tall towards it." "There was a tendency to take long breaths."

Bi. "I changed from nose to cheek."

H. "There was almost a tendency to lean and to expand upward." "There was a muscular tendency of relaxation; it becomes almost a desire to maintain the experience."

Examples of line 6 are:

D. "There was a general feeling of P throughout the entire body."

H. "There was a general liveliness of pressure-quality, an expansive tendency in the abdominal region; the whole muscular system from the hips up was involved in it."

The Auditory Experiment. The same instruction was used in this experiment, except that the first sentence was again made appropriate to the change in stimuli.

Two adjoining rooms were used. In *E*'s room there was a large funnel-shaped horn. From the small end of this horn a 1.25 cm. rubber tube extended through double doors into *O*'s room. While one *E* operated the stimuli, a second *E* moved the free end of the tube gradually away from *O*'s ear.

The stimuli were: pleasing musical selections (victrola records); an unpleasant record made by causing the record to revolve from a point other than the center; chords and discords played on tuning forks; the scratching of the finger nail on sand paper; and the dropping of brass rings.

During the last half of this experiment the *O*'s were definitely instructed "voluntarily to inhibit any movement or tendencies to movement." The introspections marked * were taken from reports under this negative instruction. It will be noted from Table III that Bi reported marked tendencies to movement or actual movement in 14 out of 16 P cases; it is also interesting to note that here he reports two cases with organic stir.

TABLE III

Observer	H	Bi	D
Total No. of Reports	35	31	35
Total No. of Reports classified as P	22	16	28
No. of P reports characterized by definite seeking movements or tendencies to movement	15	14	15
No. of P reports characterized by secondary seeking movements, such as change in breathing, expansion, etc	17	6	23
No. of P reports with organic stir	12	2	15
No. of P reports with gross observed movement	12	8	12

Examples of the kind of introspection that we have classified in line 4 of the table are:

D. "Inclination to follow stimulus." * "Inclination to follow stimulus was repressed because of instruction." "There was a tendency to move the trunk in order to follow the stimulus as it moved in different directions, going away from me and then returning."

Bi. "There was pursuit from two causes, from P of the music itself, and from the effort to hear every note in order to identify the selection." "I felt strain in my neck as if I were following the tube backwards and upwards." "That was very P. The only U part was being compelled to pursue the tube in order to maintain the P experience." * "I found myself wishing that I could stretch out my ear. I am sure that my eyes turned in the direction of the stimulus two or three times. I am sure that I found myself starting to move." * "I could feel a pretty marked incipient tendency toward movement in order to hear the tone better." * "Once or twice there was a definite tendency to incline toward the door. This was when the music became weak." * "I had a great many ideas of movement of pursuit or tendencies to pursuit. Probably these were ideated movements and

are just pictorial of what I should have to do in order to follow the stimulus if actual movement were not inhibited."

H. "There was a special tendency to reach toward the stimulus when it became very weak." "The dying out of the tone brought an impulse to move head, this movement was noted after its beginning." "There was a muscular impulse to turn toward or adjust for the having of the experience." "I had an impulse to turn toward the sound source." * "There was a tendency to move my head which was corrected after the movement was made."

Examples of line 5 are:

D. "I took long breaths and relaxed." "There was a tingling in my finger tips. I closed my eyes." "I swayed." "There was a certain amount of rhythmical kinaesthesis." "There was a tendency to take deep inhalations. I always have this tendency with P."

Bi. "There was a tension in my neck of the kind that would turn my ear toward the tube." "There was a P kind of kinaesthesis." "Two or three times I found a tendency to rhythm."

H. "There was relaxation with slight expansion and deep easy breathing." "I breathed rhythmically." "There was an expansive tendency."

Examples of line 6 are:

D. "I experienced a sinking sensation in the pit of my stomach." "My organic system was very relaxed." "My stomach felt heavy."

Bi. "There was perhaps a slight organic 'stir' of P." "There was a kind of organic thrill."

H. "I experienced a diffuse quality that was bright and lively." "There was a diffuse bright pressure." "There was a diffuse expansive tendency in the abdominal region especially, but also in the body in general, a very weak bright-like pressure."

General Conclusions

We find that in 55.7% of the total number of cases classified as P all our *O*'s reported definite movement or tendencies to move; while in 66.6% of the cases secondary reactions characteristic of pursuit or desire 'to maintain the experience' were also recorded. When we note that 28.6% of the cases reporting secondary seeking characteristics were found in P cases other than those classified in line 4, we therefore determine that definitely seeking or maintaining reactions to P stimulation are found in 84.3% of the total number of P cases; whereas Young found no direct involuntary tendencies to movement, or actual movement, reports only 11 cases of the secondary type, and classifies even these as voluntary.

The results obtained from our introspections are completely verified by the graphic records taken during the Olfactory Experiment. We noted, however, that the upward direction (with U-withdrawal) took place immediately, and was steeper than the downward direction. The lines correlated with P were of a gradually sloping nature. This result verifies Young's conclusion that the reaction to U is quicker and more intense than that to P. As the *O*'s varied widely in the intensity of their reaction to the various stimuli, a quantitative statement of the amount of change would be inadequate. In general, the younger *O*'s made more pronounced reaction to both U and P stimulation than the older. Since these reactions were entirely involuntary, there can be no doubt that they are characteristic of P, and were not occasioned by instruction or by laboratory predisposition.

In the case of taste, it is an everyday fact that the stimulus is ejected if U and retained if P. Young does not tell us the results of his chocolate-drop experiment.

It seems from our experiment that, if 'contraction' is a better term for describing the involuntary response to U, 'expansion' is a better term for describing the involuntary response to P. With intense U we found a definite withdrawal, and with weaker U a 'shrinking' of the organism, a desire 'to roll up into a ball,' a 'contraction,' a 'dull pressure quality,' etc. These organic tendencies all carried the meaning of withdrawal. Likewise on the side of P we found that there was either (1) actual pursuit or tendency to pursuit, if the stimulus was sufficiently intensive and there was danger of losing it; or (2), if the stimulus was weak or there was no danger of losing it, an expansion of the organism, a desire 'to get more of the experience,' 'to relax *toward* the stimulus,' 'to relaxation, yet with a rather high degree of muscular tonus,' etc. H has characterized his organic sensations as bright-like, lively, glowing. All these organic sensations carry the meaning of pursuit. In both P and U the intensity of the affective quality is an important determinant of the expression which the response will show.

We believe, therefore, that Young's results are derived from the particular situation under which he placed his *O*'s. As they were seated throughout his experiment in a comfortable chair, they were already relaxed, and therefore it was easier for them to be 'passive and receptive' with P than with U stimuli. Their bodily comfort was a source of constant P stimulation, whereas our *O*'s were indifferent to their position.

A second and more important criticism of Young's method is its inadequacy for producing P responses. If an *O* has a P stimulus placed under his nostrils, there is no incentive for him to make seeking movements, unless *E* begins to withdraw the stimulus. Indeed, when we repeated the Olfactory Experiment under Young's conditions and instructions, we found no definite seeking movements or tendencies to move; though the relaxation reported always had an expansive component.

There is no doubt that the most natural response to U is a movement of withdrawal. The direct response of the organism to P is, as stated above, either relaxation *with a certain degree of expansion*, if the stimulus is weak or stationary; or, if the stimulus is intense, and if the source of the P is withdrawn, a definite activity of pursuit or of tendencies to pursuit.

LIV. The Integration of Punctiform Warmth and Pressure

By R. S. Malmud

This study is a companion to the investigations made by Cutolo[1] and Tung.[2] Cutolo determined the psychological result of the simultaneous stimulation of warm and cold spots; Tung did the same thing for cold and pressure spots; and we have extended the inquiry to warm and pressure spots.

The method of this experiment was, in general, that of Tung, and falls into two parts. In the first part we stimulated a spot that was responsive both to warmth and pressure, and asked our *O*'s to report the course of the subsequent experience. In the second part we first aroused a warmth (without pressure) and then excited a neighboring pressure spot, and asked our *O*'s as before to report the course of the resulting experience. As apparatus for the former we employed the thermaesthesiometer used by Tung, but mounted on a mechanical stimulator instead of Thunberg's spring-balance device, and with warm instead of cold water running through the point. The source of the warm water was a tank about 1m. above the apparatus, heated by a Bunsen burner, and connected by a syphon with another tank of cold water; the syphon served the double purpose of permitting a smaller body of heated water, and at the same time of maintaining it at a fairly constant temperature by adding cold water at the same rate as the heated water flowed out. Thermometers were placed both in the heated tank and at the outlet; by these means we were able to determine the loss in temperature between source and outlet; and knowing the total length of the conduit and the position of the thermaesthesiometer within it, we were able to calculate accurately enough for our purposes the temperature of the water at the stimulus. This was on the average found to be 40.5° C, with a range from 38.5° C to 43° C. The temperature of the research room varied between 21° C and 24.5° C. We worked on the skin of the forearm; four or five reliable warm spots were marked by tattooing, and in an experiment the arm rested in a plaster cast. The instruction given to the *O* was as follows: "You will be given a cutaneous stimulus, and you are asked to report the course of the resulting experience particularly as regards its quality and intensity."

As apparatus for the second part of our experiment we used the electric thermaesthesiometer devised by Dimmick for his experiments on the localization of warmth without pressure.[3] This was fastened in the mechanical stimulator, and in an experiment was lowered over a warm spot to a distance of about 5 mm. above the skin; and then, when warmth was reported, was raised 5 or 10 mm. to avoid the arousal of heat. For the pressure stimulus we had intended to use a von Frey hair; but because the radiant heat so curled the hair as to make it useless, we substituted a wooden cylinder, 2.6 gr. in weight, and with a smooth spherical point about 0.5 mm. in diameter. This,

[1] F. Cutolo, Jr., this Journal, xxix, 1918, 442.
[2] S. Tung, *ib.*, xxxii, 1921, 421.
[3] F. L. Dimmick, this Journal, xxvi, 1915, 142-157.

held loosely in *E*'s hand, was applied over a hair-bulb adjacent to the warm spot. Since the crucial part of an experiment was the moment when pressure was added to the warmth, we first aroused the warmth, then gave the signal 'now,' and immediately thereafter applied the pressure stimulus. The instruction was: "After the signal 'Now' report any change in the quality or intensity of the experience." The *O*'s were Professor Weld (We), Mrs. A. K. Whitchurch (Wh), a graduate student, and I. Bershansky (B), an undergraduate specializing in psychology.

Results of the First Method. When a spot on the cutaneous surface that responded both to warmth and to pressure was stimulated with a warm-pressure stimulus, the general course of the experience as described by all *O*'s was, first, a weak pressure (contact), and then a diffuse warmth which with fluctuations gradually disappeared. At times the pressure faded out as warmth appeared; at other times pressure and warmth were present together. In the latter case the pressure was localized either as a core within an area of warmth, or as of the same area and place as the warmth. At times also one of the qualities was focal and the other marginal, and the one focal quality fluctuated in intensity independently of the other; at others both qualities were focal, and they fluctuated together in intensity. When the latter experience occurred, the two qualities were reported as fused, and were characterized as a warm-pressure, or a pressure-warmth. Since these fusions were found also in the second series of our experiments, we shall reserve their further description until we have discussed the results of that part of our investigation.

Results of the Second Method. The course of the experience when a warm spot was first excited without touching the skin, and then pressure added by stimulating a neighboring pressure spot, was in general the same as that obtained by the first procedure. The fact that in these experiments the warmth was already focal when pressure appeared seems to have had no influence upon frequency of fusion. Since, in Tung's experiment, the fusion of cold and pressure appeared more readily when attention was directed to the cold, this result of ours was contrary to expectation. Another difference in this series, and one resulting directly from the change in method, was that of relative localization. In some instances pressure was felt at the side of the warmth, and in others entirely without the area of warmth; in still others, however, it was localized, as in the first experiments, either as a core in the center of warmth, or as coincident in area and place with the warmth. In the first two of these cases fusion never occurred.

The Conditions and Nature of the Fusions. From an analysis of the introspective reports from both series of experiments it seems that fusion never takes place unless both pressure and warmth are of the same degree of clearness, and unless both are localized in the same area, although pressure may be less in extent than warmth. In some (and perhaps in all) cases of fusion, the components fluctuate or decrease together in intensity; a change in the intensity of the one or the other of the two qualitative components disintegrates the fusion. As we have seen above, the fusions are of two kinds: a warm-pressure, and a pressure-warmth. In the former, either a small core of pressure seems to radiate the warmth, or if the areas are coextensive the pressure is more intensive; it has, furthermore, the object-meaning of "being touched by a warm blunt object." In the latter, the areas are

always coextensive and the warmth is more intensive than the pressure; and it has the object meaning "of warmth pressing down," or "a large, warm, diffuse, yielding body touching the skin." Typical descriptions of the two kinds of fusion follow.

Warm-pressure. Wh. "There was a time at the beginning of the experience when pressure and warmth fused; there was a core of hard pressure from which warmth radiated; it was as if a large hard thing were touching me." "Then they [warmth and pressure] fused; it was like a large warm blunt point pressing." "A big warm blunt thing pressing; then both warmth and pressure decreased in intensity, and then suddenly, instead of having the blunt warm thing touching, I had a warm area with a very weak pressure in the center."

We. "Thereafter the pressure was felt as a warm-pressure; by that I mean a pressure to which the warmth seems to adhere as its quality; both are there together, but the pressure is the more intensive." "Then the two qualities fused to a warm-pressure; the experience was as if I were touched by a warm object."

B. "Pressure and warmth were at the beginning fused; both had the form of a pin-head but were larger. After the pressure had disappeared the warmth became more filmy and diffuse, and without definite form."

Pressure-warmth. Wh. "Pressure and warmth were felt simultaneously; pressure seemed to cover the area of the warmth instead of being in the center; it was a fusion, as if warmth were pressing." "Pressure seemed as large as the warmth; there seemed to be a fusion of the two, and the intensity of both diminished and faded out together without fluctuation; the pressure that at the beginning was small fused with the larger warmth when the latter came."

We. "The warmth had an insistence about it that was like a deep pressure, weak in intensity; a pressing warmth." "I can characterize it only as like a warmth from a warm point; *i.e.*, a warmth that was pressury in quality; it was not a warm pressure, it was not as if I were touched by a warm object."

B. "Pressure seemed to fuse with the warmth which became intense but diffuse and filmy, and seemed to flow out of the skin." "The contact seemed like a big warm thing touching me; the pressure got weaker, but warmth remained."

Of equal importance for determining the conditions of fusion are introspections which describe cases of no-fusion. *Wh*, for example, says: "The pressure was spatially unrelated to the warmth; I could not localize the pressure within the warmth; they were two separate experiences, temporally but not spatially related;" and *We* likewise reports: "The contact was at one side of the area of warmth; it inhibited the warmth; but when I searched for the latter it was there at very good intensity; the two impressions were not experienced together;" and again: "Pressure is not felt as a warm-pressure, but rather as a pure pressure within an area of warmth."

The conclusion that the fusion of warmth and pressure is only a pressure-warmth or a warm-pressure was, at first, reached by us with some consternation. Since Tung had found the fusion of cold and pressure to be cold-wetness, we had confidently expected to find warm-wetness. This expectation was further justified by a result of Bentley's, who in experiments with areal warmth and pressure had obtained a few reports of warm-wetness.[4] In the face of our own find-

[4] M. Bentley, this JOURNAL, xi, 1900, 414ff.

ing it occurred to us that perhaps our *O*'s did not know the experience of warm-wetness. We, therefore, with two of our experienced *O*'s performed a few simple experiments like dipping the finger into warm water, or causing a drop of warm water to fall upon the back of the hand; but our *O*'s still failed to report warm-wet; they had, it is true, a perception of liquidity, they 'knew' that the finger was immersed in water and must therefore be wet; but there was no experience of wetness; and the only experience from the drops of warm water was warmth and contact. We then, with four *O*'s (Professor Hoisington was added to our group), repeated the significant parts of Bentley's experiment. His conditions were, we believe, duplicated in essential details. Our *O*'s were asked to close their eyes during the time the stimuli were in the room, and after signals to report the qualitative nature of the experience felt at each one of the three stages of the experiment. The results are contradictory. Two *O*'s, *We* and *H*, still fail to describe any experience as warm-wetness; the other two, *Wh* and *B*, report warm-wetness frequently. From a study of these reports it is plain, however, that the experiences which carried the meaning of warm-wetness, for the two latter *O*'s, are not the same. For *B*, when the finger is immersed in warm water (39° C.), warm wet comes immediately; is stable except with 'shift of attention,' when it changes to 'dry' warmth; is localized wherever warmth is localized; is felt when finger or stimulus is not moving; and is not distinguished from liquidity. Whereas for *Wh*, under the same conditions, warm-wet does not come immediately; it is uncertain, instable, has short duration, and fluctuates; it is localized under the finger-nail or on the ball of the finger, or again, as creeping up the finger; it is felt only with movement of the stimulus or of the finger; and, finally, it is distinguished from liquidity as 'less thick.' Our only solution of this diversity of report is that *B* has fallen into the stimulus-error, and that *Wh* gives the meaning of warm-wet to some peculiar or atypical experience. In view of the reports of the other two *O*'s, which are in complete agreement, and also in the light of our main experimentation, we incline to the opinion that there is no typical experience to which the meaning of warm-wet attaches, as there is for cold-wet.[5]

Summary. The simultaneous stimulation of warmth and pressure spots may or may not result in a fusion of the qualities of warmth and pressure. Fusion occurs when the two qualities are equally clear and, at the same time, are spatially coincident. The fusions are characterized either as warm-pressures or as pressure-warmths. The adjectival quality is the less intensive, and seems to adhere to (or to form a constituent of) the other.

There seems to be no typical experience of warm-wet.

[5] Bentley's *O*'s did not, apparently, distinguish wetness from liquidity. See *op. cit.*, 424.

BOOK REVIEWS

Erlebtes und Erkanntes. By WILHELM WUNDT. *Stuttgart,* A. Kröner, 1920. Pp. xii., 399.

What a man has made of his life, Wundt says, is measured by the ratio of *Erkanntes* to *Erlebtes,* of the lessons learned from experience to the data of experience itself. The measure, taken in the rather special sense in which Wundt intends it, gives him a sufficiently large vital quotient; few of us would think of using a box on the ear, received in the first year of schooling, to illustrate the Law of the Continuity of Consciousness! Throughout the book, indeed, *Erkanntes* is far more in evidence than *Erlebtes,* and again and again the reader is diverted from some simple item of narrative to a technical disquisition which contains all the subtlety of Wundt's mature thought and shows all the intricacy of his mature style. Since we are familiar with the contents of Wundt's principal works, I shall pass over these scientific homilies and shall confine myself in the present notice to matters of autobiographical fact and to certain questions of genesis.

First of all, however, I must remark that whatever else Wundt learned in the course of his long life he had not learned fully to know himself. He tells us in the Preface that the cast of his mind was political, and its leading motive a desire to further the interests of state and society. There is, no doubt, a grain of truth in this characterization; Wundt was neither the typical man of science nor the typical philosopher. In the main, however, the statement derives from a very natural illusion. Wundt's boyhood was a time of marked political disturbance; he had worked off the superfluous energy of his early manhood in various social and political undertakings; and the closing years of his life were spent in the shadow of the Great War. The end thus harked back to the beginning, and the Karlsruhe episode took on in memory an undue importance. The illusion is, surely, natural enough,[1] and furnishes us with some thirty pages of interesting and instructive reading. It is, none the less, an illusion, and one to which Wundt himself elsewhere supplies the needed corrective. Every man, he declares, really lives a number of lives, which are more or less closely connected, but which run their separate courses to the end. His own political bent belongs, without question, to a partial and subordinate life whose brief periods of dominance are separated by long stretches of scholarly absorption.

In a recent paper I gave a scanty outline of Wundt's life. This outline can now be filled out, and at one point, where I made an unlucky guess, corrected. At the time when Wundt's schooldays began his parents were living at Heidelsheim. Here he passed two years in the *Volksschule,* and thereafter, until he entered the *Gymnasium,* was taught privately by his father's *Vikarius.* Wundt was a solitary and imaginative boy, averse from games and from school-work, much given to day-dreaming and, later on, to miscellaneous reading of poetry and historical novels. He was the fourth child in the family; but two children had died so early that he had no remembrance of

[1] Readers of the Leibniz-brochure of 1917 will not think it fanciful if I suggest, as a secondary source of Wundt's belief, the parallel between himself and Leibniz.

them, and his surviving brother, eight years older than himself, was away at school, a member of his aunt's household in Heidelberg. He thus passed his early years "ohne Geschwister und ohne Mitschüler."

His tutor, to whom he was so deeply attached that he followed him for a year to Münzesheim, seems to have been anything but strict and to have taught him but little. Wundt was able, nevertheless, to enter the *Gymnasium* at the neighboring town of Bruchsal, where he spent an unhappy year,—whence, indeed, he ran away, only to be escorted back by his mother. The misfit was so plain, however, that his parents determined to remove him to Heidelberg. Here (and not, as I had guessed, at Mannheim) he attended the *Gymnasium* from his fourteenth year until the time came to exchange school for university. The year of entry was overcast by the death of his father; the later years passed happily. Wundt was taken in hand by his brother, who saw to it that he worked with some steadiness; he made friends, and was pledged to membership in a student *Korps;* following the example of his uncle Arnold, a professor in Tübingen, he devoted himself enthusiastically to the study of the anatomy of the brain; he read widely, and on long walks with his fellows discussed what he had read; in a word, he lived the normal, companionable life of his age and environment.

Required, now, to make choice of a career, Wundt after some hesitation decided upon medicine. He had no special call; but he wanted above all things to get away from home; he had been interested, as we have seen, in brain anatomy; and as a beginning student of medicine he could, with some show of reason, migrate to Tübingen for his uncle's instruction. The year 1851-2 saw him, accordingly, a student at that university. In the summer of 1851 he returned to Heidelberg, with increased knowledge of brain anatomy but with very little beside, save the conviction that nature had not meant him to be a physician. He was forced afresh to a decision, and his choice fell on the new experimental science of physiology. His first Heidelberg year was spent upon preparatory studies in mathematics, physics and chemistry. Bunsen's lectures had well-night seduced him into preferring chemistry to physiology; but he remained true to his considered choice, and compromised only in so far that his first scientific publication—duly listed in the JOURNAL's bibliography under the year 1853—dealt with a problem in physiological chemistry. During the second year he studied clinical pathology and pathological anatomy. A prize-essay, in preparation for which he performed, at home and with the help of his mother, a series of vivisectional experiments on the rabbit, brought him a word of commendation from Johannes Müller. The third year —there must have been a third medical year, if later dates are to square—seems to have been devoted to the special medical subjects; I cannot make out that Wundt marks it off, definitely from the year preceding. At the end of this third year, as it would appear, he took his *Staatsexamen,* and came out first in all three divisions: internal medicine, surgery and obstetrics.

In spite of his success in examination, Wundt was still not disposed to attempt a medical practice. He accepted a semester's position as substitute assistant in Hasse's Clinic, and became so keenly interested in his work there that he again had thoughts of deserting physiology, this time for pathological anatomy. In addition to his regular duties, he prepared an experimental thesis for the doctorate of medicine at the University: it was published in 1856. At the expiry of his term of service he determined, with a little money in hand, to go on a

physiological pilgrimage to Berlin, and there to utilize a final semester in work with Johannes Müller and du Bois-Reymond. He entered the University of Berlin in the spring of 1856; began at once a couple of experimental investigations, the results of one of which led to publication; and came back to Heidelberg, his student-days over, in the late summer of the same year.

Wundt's next step was to offer himself as *Privatdozent* in physiology to the medical faculty of his own university. There was very little formality in the matter: the doctorate thesis was accepted as *Habilitationsschrift*, and examinations were dispensed with. He straightway advertised a six-hour course in physiology, with demonstrations and experiments, which attracted an audience of four. But he had undertaken too much. A series of hemorrhages cut short the lectures and the beginnings of research; physicians and family despaired of his life; and it was a full year before his health was thoroughly restored. His experiences and reflections during this illness had a permanent effect upon his philosophy and his idea of the relation of philosophy to science.[2] When he was able to take up his work again, apparently early in 1857, he busied himself with an experimental study of muscular contraction, in continuation of an *Arbeit* suggested by du Bois and begun in Berlin. The book, Wundt's first book, came out in 1858,—dedicated to du Bois, published by Vieweg, "mit vorzüglichen Abbildungen der Apparate und Methoden ausgestattet;" outwardly as inwardly a joy to the heart of its young author. But books have their fates: and this book was destined, since it did not please du Bois, to issue stillborn from the press. At first, Wundt says, he was distressed; then, resigned. Later he began to see the humor of the situation, and later still he forgot the whole affair. Now, in his old age, he reviews his early work, and finds it, on the whole, very good.

When Helmholtz was called to Heidelberg in 1858, Wundt offered himself as assistant, and was accepted. Neither Helmholtz nor Wundt had any very precise idea of what the duties of an assistant might be; Helmholtz vaguely suggested microscopy, which Wundt did not affect. The Government presently cut the difficulty by requiring all students of medicine to take a semester's work in the physiological laboratory; and Wundt accordingly found his mornings, from 8 to 12, given over to elementary instruction and indifferent students. He held out for some years, but finally resigned the position (the story of his dismissal by Helmholtz is pure myth) to devote himself to the writing of his Physiology and Medical Physics. As to his relations to Helmholtz, it is hardly an exaggeration to say that they were non-existent. Helmholtz was as silent in fact as the great Moltke is in legend, and Wundt, the assistant, did not venture to ask his professor questions; and though Helmholtz unbent in society, he unbent for social relaxation, and not for the discussion of scientific problems. It was unfortunate, too, that both Helmholtz and Wundt were now occupied with the same subject, sense-perception. Wundt in two passages comments a little regretfully on this coincidence; and there can be no doubt that the community of interest, taken together with a radical difference of perspective, held the two men apart. So we have a few anecdotes, and we have a sort of apology for Helmholtz' liking of foreigners, and that is all.

[2] The incident is discussed by W. Wirth, in the memorial article in *Arch. f. d. g. Psych.*, xl. I regret that I cannot say more of it in this place.

The biographies state that Wundt gave up his assistantship in 1864 and was promoted to *extraordinarius* in the same year. Unless I have somewhere missed a sentence, the date is not mentioned by Wundt himself. He tells us that the *Beiträge* were inspired, primarily by E. H. Weber and by his own experiences of cutaneous anaesthesia in Hasse's Clinic, secondarily and more remotely by Johannes Müller. The notion of creative synthesis came to him, in connection with the problem of visual space perception, in the spring of 1858. The genesis of the *Physiologische Psychologie* is less clear, and Wundt's account of the book itself is, I venture to think, somewhat colored by his later views,—though this coloring, since it shows us what Wundt would have planned if he were writing the work anew, is of importance in its own right. The one thing we see clearly is that Wundt, during the time of his connection with Helmholtz, was steadily reading the standard psychologies of the day. He got up at 5 o'clock in winter to study Herbart's *Psychologie als Wissenschaft;* and he familiarized himself with Lotze, Fortlage, George, Volkmann, and the rest, as well as with the older psychology of Wolff and Kant. If this wider interest appears, in the light of Wundt's whole career, as a natural consequence of the original interest in sense-perception, I must still believe, as I suggested in the paper already referred to, that it reflects also a direct reaction against the anti-psychologism of Helmholtz. Otherwise we should have heard more of psychological difficulties, and more of the actual motivation of the *Physiologische Psychologie.* The book came out, as we know, in 1873-4, and its successive revisions are described by Wundt as "ein beträchtlicher Teil meiner Lebensarbeit."

We have, I take it, a like admixture of *Wahrheit und Dichtung* in the account of the genesis of the *Völkerspsychologie.* We need not question the statement that, about the year 1860, Wundt conceived the idea of adding social psychology as a sort of superstructure to experimental psychology; the preface to the *Vorlesungen* stands as witness. But Wundt in 1920 had not gone over this work as carefully as he had restudied the *Muskelbewegung.* He forgets that the theory of thought is completed in the first volume, before social psychology makes its appearance; he forgets his express repudiation of Lazarus and Steinthal; he forgets his insistence that the book is a book of individual psychology, and that he introduces the facts of social psychology only to round out this individual psychology, especially on the side of morals and religion. The fact remains, of course,—there is *Wahrheit* amid the *Dichtung,*—that the second volume of the *Vorlesungen* is largely made up of material which was later psychologized in the *Völkerspsychologie,* and that it contains the germs of later theories. Wundt's first lectures on social psychology were, however, delivered at Zurich in the summer of 1875, after the publication of the *Physiologische Psychologie;* and the *Vorlesungen* probably drew less upon special studies than upon the contents of his Heidelberg lectures on anthropology. The new course seems also to have owed something to Wundt's recent occupation with Aristotle.

During his whole long stay at Heidelberg Wundt was busied about his titular subject of physiology. But he also became variously interested in philosophy, and as early as 1866 published his first philosophical book, *Die physikalischen Axiome.* Towards the last this interest must have been recognized as dominant by his colleagues; Kuno Fischer told him later that he had intended to recommend him to a philosophical chair in Heidelberg. The call came, in fact, from Zurich: at first, apparently after the publication of the first half of the *Physio-*

logische Psychologie, as a tentative enquiry, and a year later in definitive form,—a call to the chair of inductive philosophy vacated by Lange. Wundt was not fated, however, to remain long abroad. In May, 1875, he was offered a chair of philosophy at Leipzig. The faculty had first called Fischer, who was unable to accept. Then the idea arose, partly in view of the great age of Drobisch, of dividing Ahrens' professorship, and of securing at the cost of a single philosopher of repute two young men, the one of whom should represent the philological, the other the scientific aspect of academic philosophy. The choice fell upon Heinze and Wundt, whose youth and insignificance inspired a satirical journalist of the day to transform their names into the familiar Hinz and Kunz. One wonders how long that journalist lived! The moving spirit in the whole affair was, curiously, the astrophysicist Zöllner, a man known to psychologists both by his 'illusion' and by his unfortunate association with Slade. Zöllner had a strong leaning toward philosophy, concerned himself actively with the filling of the chairs, and was allowed by an apathetic faculty to pick his men. The Herbartians, Drobisch and Strümpell, had nothing whatever to do with Wundt's appointment,—and another historical anecdote turns out to be mere invention.

Wundt's arrival at Leipzig brings us near the end of his story. He was privileged there to enjoy the friendship of E. H. Weber, the father of experimental psychology, who had still three years to live, and of Fechner, the founder of psychophysics, who lived till 1887. From the first he aspired to organize and direct a laboratory. He started with a few rooms in the old *Konviktgebäude;* in 1892 he obtained more adequate quarters in the *Trierianum;* and in 1897 he moved back to the main building of the university, and took possession of the new-built Institute. In 1913 an extra story was added to the *Paulinum*, over a great part of the Institute proper, and was assigned to *Völkerpsychologie*. Other overhead construction was planned, with a view to a special section of psychophysics; but the war intervened, and psychophysics is now temporarily housed in some of the rooms intended for *Völkerpsychologie*. The three-fold Institute is therefore not yet complete, though it is all laid out and, apparently, promised; and if Wundt did not live to see his early dreams come fully true, we must remember that the scale of actual accomplishment far exceeds anything that he could have hoped for; the half has again proved to be greater than the whole.

In thus restricting myself to biography and to notes on Wundt's principal psychological books, I have left out of account what to most readers will be the really interesting parts of his last work. Beside the scientific homilies to which I have referred we have vivid sketches of men and their manners, of universities and cities; pregnant essays upon certain large questions of education; and a final section on the practical ethics of the Great War. Wundt is animated by a fervid and somewhat narrow patriotism. It is a thousand pities that he never visited England or America; the Anglo-Saxon genius is as foreign to him as it was familiar to Helmholtz, and he writes of the two countries with all the assured superficiality of the outsider who has facts but no perspective. His attitude to the future of Germany is that of chastened but convinced optimism. The *Volk* has been purged in three fires: the Reformation, the Thirty Years' War, and the Great War. It must and will, after the throes of present revolution, return to its native idealism and on that basis reconstitute the German State, which

will then play its due part in the furtherance of civilization.—But this and many other passages of *Erlebtes und Erkanntes* demand fuller consideration than is allowed by the limits, however generously drawn, of a book-review.

Fuller consideration of the book as a whole, taken together with many other books, is also needed before *der innere Zusammenhang* of Wundt's life, which his preface leaves us to make out for ourselves, can be brought into clear light. We see that he was one of those not uncommon natures that combine a large capacity for day-dreaming with an obstinate persistence in the day's work. We see that he had a steady dislike of routine imposed from without, though he presently laid a scholar's regimen upon himself; he shrank from the life of a schoolmaster or medical practitioner; he escaped to Tübingen and clung to Heidelberg that he might work and play at his own sweet will. We see that his endowment was general and not special; with better schooling, he might have become a philologist; as it was, he might have been a chemist or a professor of pathological anatomy, or might have remained a physiologist and attained to eminence in that field; and if he was not in fact a politician, he would have made a very good minister in a bureaucratic government. Apart from these generalities three things, in particular, have impressed themselves on me as I have read. The first is Wundt's quite unusual combination of the experimentalist with the generalizing logician. Few men of science, even great men, hold the balance of interest as truly as Wundt held it. The second thing is his extraordinary power of 'cram.' He could keep available an extended and intricate subject-matter for as long as he needed it, and could then drop it, and turn fresh to a new topic. I know no better word than 'cram' to use for this capacity, but it was cram carried to the *n*th degree. The third thing—a character that I have called attention to in my biographical paper—is Wundt's imperative tendency to systematize the unripe. He was not a systematist of pure blood; for that, his thinking (if I may say so with all respect) was too turbid, too little clear; neither, of course, was he the mere encyclopaedist; he was continually essaying system. All these points are illustrated in his sixty years of psychologizing. They do not account for the psychological trend: if we are ever to understand that, we must know more of *Angelegtes*, and relate it both to *Erlebtes* and to *Erkanntes*. E. B. T.

L'année psychologique, vingt et unième année (1914-1919), ed. by H. Piéron. Paris, Masson et Cie., 1920, pp. xii, 522. Price 35 fr.

Psychology welcomes the reappearance of *L'année psychologique*. Founded in 1894 by Alfred Binet, it appeared regularly every year until the 20th volume had been published in 1914. Then the war intervened, and for the next six years publication ceased. Now it appears once more as a sign that French psychologists are again at their habitual work. The '*Année*,' as it is familiarly called, has from the first occupied an unique place among psychological periodicals. It was designed, as its title implies, principally as a year-book of psychology. It was the founder's original plan to divide the book into three parts: the first to contain original articles, essays and experimental investigations (chiefly from the laboratory of the Sorbonne), and general reviews of important topics written by experts in their special fields; the second to consist of digests of all the significant books and monographs of psychological interest which had appeared during the year; and the third to be a psychological index for the year. With this

schema it was his policy to cover as much of the whole range of psychology as possible for the twelve-month period. Both schema and policy were maintained until 1904, when the policy was broadened to include occasional articles and reviews by specialists in fields adjoining psychology,—particularly in cytology, in the anatomy, physiology, and pathology of the nervous system, in mental pathology, anthropology, criminology, the pedagogy of the normal and abnormal child, ethics, and philosophy. In order to make room for this extension of programme the psychological index was dropped. Then, in 1908, came a still more marked change in policy; for in that year Binet announced his purpose henceforth to devote the major part of the *Année* to problems in practical and social psychology. This plan was put into effect and adhered to until his death in 1911. In 1912 the *Année* was edited by Larguier des Bancels and Th. Simon, and consisted entirely of *mémoires originaux*. The following year, 1913, the editorship was assumed by H. Piéron, Binet's successor at the Sorbonne, who has in general followed the policy laid down by Binet in 1904.

The high quality of the *Année* throughout its history is beyond question. Many of the research articles, particularly those written by Binet, alone or in collaboration with Simon, have had a decided influence upon the subsequent history of psychology. Furthermore, its pages have frequently been the means of publication for the best contemporary psychologists of France, for Beaunis, Ribot, and Henri (all three of whom were at one time or another co-editors with Binet), for Bourdon, Féré, Foucault, Piéron, Janet and Wallon. But the uniqueness of the *Année* lay in the nature of its general reviews, and in the character of its *analyses bibliographiques*. The former possessed an insight and an authority which still make them valuable as summaries of the period which they reflect; the latter had as ideal so thorough an abstraction of the principal features of method and result as to make it unnecessary for the reader to revert to the original articles. It goes without saying that this aim is in many instances impossible of fulfillment; but with the frequent reprinting of tables and curves, and of figures of apparatus, the digests, made for the most part by the editorial staff, attained as a whole a quite unusual completeness and reliability.

The present volume is a monument to the industry and enthusiasm of Piéron. He and his students have contributed three of the experimental studies, and he himself has written two of the reviews, nearly all of the 600 *analyses bibliographiques*, and the *chronique;* and as a result he, with the aid of his collaborators, has succeeded in producing a volume that is a worthy representative of the series. The first of the *mémoires originaux* is an experimental study of expectation (*problème de l' attente*) from the Sorbonne laboratory by Mlle. Morand; there follow experimental investigations of auditory space perception (*Recherches sur les perceptions spatiales auditives*) by B. Bourdon, of the persistence of an acquired ability (*La persistance des aptitudes acquises*) by M. Foucault, and of a comparison of the memory for meaningless forms with that for numbers (*Recherches comparatives sur la mémoire des formes et celle des chiffres*) by H. Piéron. Following these is an essay on the psychoneuroses (*Essai psychologique sur les psychonévroses*) by M. Mignard and A. Gilles; then comes another experimental study from the Sorbonne laboratory on the cor-

relation of apprehension with memory (*Capacité d'apprehension; rapidité d'acquisition et puissance de rétention de souvenirs bruts: Recherches de corrélation*) by Ot. Vlaïcou; and finally an essay on the types of aesthetic ideas (*Les types d'idéation esthetique*) by M. Grzegorzewska. One cannot escape the impression that, with the exception of the essay on the psychoneuroses, and the investigations of Foucault and Vlaïcou which are perhaps still typical of research in educational psychology, these studies are a little out of tune with present-day psychology. Morand, for example, is content to rest the case of expectation with the conclusion that it is a conscious attitude of the intellectual sort; Bourdon does not bring his results into line with recent experiments on the localization of sound; and Piéron would hardly conceive of his problem today as he did in 1912 when his work began. It is only fair to say that, but for the war, the list would probably have appeared in 1915. There is, however, a result of Bourdon's that is more opportune. He concludes that, aside from intensity, the principal cue to the localization of sound both in distance and in direction is a character which he calls degree of *netteté*. He does not think that timbre in its own right can have much influence on localization. The degree of *netteté* is conditioned in part upon the distance of the sound, in part upon the position of the head relatively to the source of sound, and in part upon differences in timbre (the telephone click is more *net* than the sound of a whistle, and the whistle than the sound of a tuning fork). The psychological nature of *netteté* is, however, not made explicit; other descriptive expressions are clear, clean (*net*), pure (*franc*), distinct (*clair*); "the sound of a whistle," Bourdon says, "seemed to me less soft, less muffled (*moins flou, moins étouffé*) when it was near than when it was far." Although the general significance of these terms is obvious, it is also clear that further analysis is necessary. Whether Bourdon has discovered a phenomenological basis for the localization of sound more study alone can tell; but it would seem that he has shown a way to experimentation that should clear up a long-standing problem.

The Notes and Reviews of this volume of the *Année* consist of a short paper on the *correspondence of intelligence with instinct*, by E. Rabaud; a review of more than 100 titles on the *psychoneuroses of the war*, by H. Wallon; an account of the work done during the war by England, France, Italy and the United States on the examination of aviators (with a bibliography of 67 titles) by H. Piéron; and a report, also by Piéron, of the meeting of the newly-formed psychological section of the French association for the advancement of science held at Havre, July 27-29, 1914. The *analyses bibliographiques* represent a brave attempt to report the principal studies in psychology and neighboring fields during the years of the war. But they are limited principally to American, British, French, Italian, and Swiss sources; with the exception of 7 titles from Pflüger's *Archiv*, no Austrian, Danish, German, or Swedish papers or books are mentioned. The principal parts of the *Chronique* are a note on the part played by psychology in the war; the necrology for the six years (given, unfortunately, without dates); the events of the period, particularly as regards the vicissitudes of psychological periodicals during the war; and finally a refutation of the statement made by an American psychologist that psychological publications in the French language are, in the light of a statistical study, on the decrease. With an author's index the volume is brought to an end. H. P. W.

Vom Organismus der Sprache und von der Sprache des Dichters. Zur Systematik der Sprachprobleme. By MARGARETE HAMBURGER. Leipzig, Felix Meiner. 1920. pp. vii, 186.

The author finds such days as these well spent in taking measure of the cultural attainments of one's country. Her own contribution is to be a study of speech, since a people's speech parallels its intellectual development; and her particular concern here is the aesthetic side. But two hundred words give small space in which to do justice to one's mother tongue in all its aesthetic aspects, especially when allowance must be made for the repetition and overlapping of a style which is not succinct and an organization by no means compact. And the author's range is ambitious: theories of the origin of speech, the history of its development, an estimate of it as art and as a medium of art, a formal criticism of poets' language are her main headings; and she touches upon other problems, as the possibilities of translation, the existence of synonyms, the nature of genius. Consequently the theories are not always given the evaluation which makes such summaries most worth while; problems are dismissed with the lining-up of authorities, or with a mere statement of opinion; and generalizations are couched in terms which must lead some readers to hope that they were written more easily than they are grasped.

However, the point of view is clear enough,—and incidental in its exposition are pleasing passages of literary criticism. Speech in its beginnings is more than a vehicle of information: it is always a sign that men are not wholly absorbed in satisfying practical wants. And it is, too, at that level, more than the mere play of imitating nature's sounds. The artistic impulses which make poetry are at work when the speech of a people is formed; the early speech is poetry. In it, and even here and there in its descendant, our own talk of every day, we may expect to find the very stuff which we are accustomed to attribute to the poet's peculiar gift: rhythm and rhyme, and the figures of speech. Metaphor, indeed always takes a large part in the making of a language. It intensifies the impressions of one sense by bringing to them those of another. It names new ideational experiences which have no counterpart in expression. And here it is in the nature of metaphor that it conveys the meanings in imaginal form, and keeps always in the hearer the sense of the image. But its intention never is to illustrate; its fitness consists in the unity of its effect: its various images do not become explicit and discrete, but rather give richly on the side of mood. "Grau, teurer Freund, ist alle Theorie, und grün des Lebens goldner Baum" is the example quoted from a later, individual maker of language.

But speech must grow along with the thought of the race. The demand which German thought in its development made of language was that it became less palpable and more abstract. The music was lost, and the metaphors faded to symbols. There is, of course, gain of a sort, and one which the author does not underrate, for the speech which carries evolved thought: it must become highly organized and variously shaded. But it comes a long way from poetry; and the poet meets a difficulty, which gives other artists little worry, in this medium which has lost so much of its vividness in serving non-artistic ends. In his remolding of it for his use, however, it comes to a new birth; and we are to consider that here the making of speech, which has been the work of the race, passes into the hands of creative individuals. Even the smaller men among them have a share in the achievement, for they help in restoring and putting into circulation the

old sensory meanings of words, and whatever the centuries may have given in the way of emotional accruals. Or in depicting situations for which current German is especially inadequate, the dialects with their rhythms and moods may be adapted. But revival is not sufficient, and at various times, in protest against the flatness of common speech or heavy tradition of literature, new language has been made. Such a spirit, with the need for a new form to match their thought, inspired the *Stürmer und Dränger*.

This is the outline of Hamburger's view. One of the questions to which it brings her, naturally, is the degree to which speech created by the poet may influence, and come into, the speech of the people. Authorities say that the very color of Goethe's language made it less usable by others; words and structure might be paralleled, but the values were his alone. Other authorities say that Goethe gave German new vigor and new dimensions which are more precious than any material enriching through new colors and tones. The author herself brings instances of words which are the gifts of artists and philosophers; and concludes in general terms that, in so far as the poet crosses the limits of his individuality to find his highest development in the whole, there is harmony between his speech and the speech of all.

Vassar College J. GLEASON

Abzählende Methoden und ihre Verwendung in der psychologischen Statistik. By O. LIPMANN. Leipzig, J. A. Barth, 1921. pp. 78.

This monograph, as its title suggests, deals with the statistical treatment of mass-results rather than with the older established psychophysical methods. The view taken is from the standpoint of the individual within a group of performances, or of the group within different performances, or of different groups within the same performance. The treatment is almost wholly graphical or tabular in nature; very little importance attaches to any single representative value except as it serves as basis for the determination of some new curve. The idea of units of measurement is implied in the taking of the data, in the computation of the single values, and in the numerous interpolations; the actual comparisons are between curves or parts of curves. We have the empirically determined curve given as the standard of reference; hence the measure of any performance is a relative and not an absolute matter. Interest, therefore, seems to attach to differences rather than to uniformities; to the results as a whole rather than to an abstracted result; to relative rather than to absolute values.

The methods and practices are those of the natural sciences and of education,—of the biologist, the mental tester, and the worker in the field of experimental education. The psychophysicist will find the monograph interesting inasmuch as it presents a mode of treatment (it treats of psychophysical problems) which differs from that of the regular psychophysical methods; and he may be brought to believe that a relative judgment, after all, is as far as we may safely go in view of the present state of our knowledge about absolute units.

Because of the complexity and the unobservable nature of the conditions which obtain in psychological work, the mode of statistical treatment given to psychology by the natural sciences is inadequate to psychological data; variations and modifications must be made. The author emphasises the importance of these reforms. No less than fifteen varieties of curves are worked out and graphically illustrated, and more than a dozen others are suggested on the analogy of those

given. Wealth in abundance! so one, at first, inclines to say; why, however, do we need a round two dozen measures for one and the same thing? To be sure, every curve or set of curves shows a somewhat different relation between the sets of data, or shows the relation in a somewhat different way.

The more fundamental forms of curve are: the curve of actual distribution, the curve of difference in actual distribution, the curve of percentage distribution, the curve of difference in percentage distribution, the curve of rank order, the curve of difference of rank order, the curve of rank order according to percentage, the curve of difference in rank order according to percentage, the zonal curve in which the point on the abscissa which corresponds to the value found for the measure of central tendency taken has zero-value in terms of the ordinate scale, the zonal curve for percentages, and, finally, curves of distribution of absolute or rank differences of scores in the total series or rank orders of these differences. It is not to be supposed that all these curves employ the crude data, or that the crude data belong exclusively to any curve or set of curves. The notion of the quartile finds extensive application. Since the significance of the difference between the upper and lower quartiles depends in part upon its relative magnitude, measures of variation take the quartile values in relation to some optimal value.

The longest section in the monograph deals with the subjects of correlation, coordination and contingency. As is to be expected, the curves of regression receive more emphasis than does the bare coefficient of correlation. Coordination resolves itself ultimately into equations expressed in terms of the quartile division. The test for contingency receives but little consideration, though it is perhaps adequate for the worker in the field. The monograph closes with a brief discussion of the problem of weighting results.

L. B. HOISINGTON

The Labor Movement: Its Conservative Functions and Social Consequences. By FRANK TANNENBAUM. New York, G. P. Putnam's Sons, 1921. pp. xviii, 259.

Only one chapter in this book falls by title within the province of the JOURNAL, that on "Labor Movement Psychology." The remaining chapters, with their doctrines of the overthrow of the "profit motive" in business, the "displacement of the capitalist system by industrial democracy" and the "rebuilding" of our "social structure" by the labor movement, the development of new industrial "political institutions" as the "inevitable" outcome of the worker's activity, all deal with matters which challenge the professional attention of others than the psychologist. If a reply is to be made to the author's persuasive prophecy that the machine will force us all to become workers, that the foes of the workers will force us all to become members of the union, and that all government will ultimately and rapidly become a form of glorified labor congress with the workers as congressmen. it will be better made by specialists in government.

In the chapter on Labor Movement Psychology, the worker is described as "propertyless," "a wanderer," "a seeker for better things" "a cog." In his work his "self and personality are not involved." His employment is "monotonous," "unbearable." He has neither the "satisfaction of a job well done" nor the joy of completing a "self-made plan." For him the present is "accidental, transitional," the future hopeful. His "drifting body tends to carry with

it a restless mind," a "mind that knows nothing of the conserving, constructive experience" of those who are stabilized, for instance, by owning their own land. "Unemployment," "industrial irregularity," "occupational accidents," "the competitive and shifting market," the "adventure of ordinary business," the advent of the machine, have all resulted in "agitation of the mind and discomfort of the body" for the worker. The industrial revolution has equalized the "imagination" of men, and has increased their "irregularity of possession." Now the poor feel "spiritually" equal to the rich. The worker wants much but gets little, and is consequently dissatisfied. Equality in all but money makes money conspicuous, and so simple a difference between the poor and the rich makes the difference "more objectionable," and "desire for adjustment more vivid." Whatever unhappiness the worker feels is the stronger because it is constant and unescapable. He is driven, and driving "suppresses personality," and suppression of personality leads to "rebellion." The intellectual and critical nature of the labor movement in itself makes trouble. "Thoughtful workers" believe the world's ills could be cured "if only there were the will and the desire" to remedy them. "The strength of this conviction is in proportion to the revolutionary idealism of the workers. This conviction leads to the conclusion that the present system is not only bad but is kept so by the perfidy and selfishness of the powers who are benefiting by the present system. It thus adds to the hatred and to the instinctive opposition against being reduced to mechanical instruments by the machine, the belief in the villainous character of the capitalists as a class, a conviction that adds contempt to hatred and leaves a constant bitterness that knows no end." "Opposition by the capitalists—an opposition that is based often upon ignorance and generally upon selfishness and class standards—makes for a constant aggravation of this bitterness." His "psychological maladjustment" makes the worker a ready prey to "emotional appeals" which offer "an easy and ready rationalization of the world and its implications." "The labor movement provides an emotional outlet. It provides room for creative activity." But it does all this "in terms of the values, functions, and problems with which the worker is called upon to deal."

Obviously, such a chapter is not meant as a contribution to psychology. The author simply passes in review the events of the worker's day and the ideas which occupy him, and records the emotional value which he deems them to possess. Psychology has come to mean merely a plausible explanation of everyday experience. It is but fair to repeat that the emphasis of the book falls elsewhere.

Cornell University H. G. BISHOP

The Origin of Man. By CARVETH READ. Cambridge, University Press, 1920. Pp. xii, 350.

This book may be characterized as a psychological interpretation of the better known facts concerning Magic, Animism, Totemism, and concerning the priests and wizards who practice or encourage those rites and beliefs. The first two chapters trace the development of man from an hypothetical primitive stock, a cross between the ape on the physical side and the wolf on the mental, which the writer calls the lycanthropoid stock. These results are highly speculative, obviously. The chapter on Belief and Superstition is an interesting application of the psychology of reasoning, with illustrations from many sources. Much discussion is directed to the problem of the priority

of Magic or Animism, with the conclusion that Magic probably comes first; but the interactions of the one upon the other are also traced through two chapters.

The long discussion of the origin of totems inclines to accept Andrew Lang's view that they were names given by outsiders to a group and gradually accepted by the members of the group themselves. Later savage explanations gave rise to many myths which vary from tribe to tribe. The writer believes that exogamy and other marriage customs grew up independently because of social needs and by natural selection, and that the connection established between them and the totems is more or less adventitious. A final interesting chapter traces the historical relations between magic and science, with the conclusion that magic did rather more to hinder than to advance the cause of science.

The book contains little new material, nor all of the very recent material, but is an important working over of anthropological results for the light they cast upon, and receive from, certain psychological doctrines. On many points it may well be recommended to the student of psychology.

University of Michigan W. B. PILLSBURY

The Psychology of Thought and Feeling. By CHARLES PLATT. New York, Dodd, Mead & Co.

This is avowedly a popular presentation of psychology from the point of view of a modified Freudian. It tends to become a discussion of things in general, with a few psychological principles serving as texts. As one might expect, instincts, emotions, and the unconscious form the body of the book. The chapters on Memory, Reason, and Habit contain pleasant descriptions of the superficial processes, with no reference to experimentally determined laws, nor attempt at fundamental analysis.

The last five chapters deal with applications: Education, Mental Ills, The Crowd, The Delinquent. In these, as in less degree in the earlier part of the book, the author appeals to the Unconscious to solve all difficulties. It is a *deus ex machina* which itself needs no explanation. The book is one of the best warnings so far of what we may expect if that tendency should become dominant in psychology. One may quote the writer's own dictum: "'Overbeliefs,' those which are held without the foundation of fact, are no more true when they come through the gateway of science than when they lack this prestige."

The book should appeal to the general public, as it is well written, with many interesting illustrations.

University of Michigan W. B. PILLSBURY

PSYCHOLOGICAL PERIODICALS

Zeits. f. Psychologie. Bd. lxxxiv., Heft. 1-3. P. Bussɛ. 'Ueber die Gedächtnisstufen und ihre Beziehung zum Aufbau der Wahrnemungswelt.' [Reports experiments, performed in Jaensch's laboratory, on three types of image chosen from the memory-continuum (after-images, images of sense-memory, memory-images proper), with observers 11 to 17 years of age. Laws of the comparative behavior of these images are made out for change of the spatial relations of subject and object, for range of concomitant field of view, for clearness, for power of suppression and resistance; and the influence of sense-memory upon perception is demonstrated.] W. Fuchs. 'Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker: i. Verlagerungserscheinungen.' [Third of the series of papers edited by Gelb and Goldstein. Errors of localisation, both relative (tendency toward the point of fixation) and absolute (shift of visual space at large), are explained in terms of attention, which is however taken not as mental faculty but as 'structurally determined reaction' in Köhler's sense.] Literaturbericht.

Bd. lxxxiv., Heft 4-6. A. Gelb. 'Ueber den Wegfall der Wahrnehmung von "Oberflächenfarben:" Beiträge zur Farbenpsychologie auf Grund von Untersuchungen an Fällen mit erworbenen, durch zerebrale Läsionen bedingten Farbensinnstörungen.' [Fourth paper of the Gelb-Goldstein series. Reports cases in which there was loss of surface colors (Katz), and all visual objects appeared filmy or filmy-bulky. Persistence of object-consciousness assured the color-constancy of these films.] H. Friedlaender. 'Ueber Gewichtstäuschungen.' [Reports experiments on the resting skin. Illusions with stimuli differing in material are due to expectation; those with stimuli differing in volume to expectation *plus* the influence of 'seen' density. The 'idea of expectation' involves a complex play of the unconscious associative mechanism, and its effect may be a positive as well as a negative (contrast) suggestion.] K. Scholl. 'Vom absoluten Eindrucke bei Schallstärkevergleichungen.' [Attempts to prove the relative (empirical) nature of 'absolute impression' by the effect of series of single (loud or weak) or paired sounds interpolated between regular series. No clear result can be made out.] Literaturbericht.

Bd. lxxxv., Heft 1-4. Festschrift zum 70. Geburtstage von Prof. Dr. Georg E. Müller. J. Froebes. 'Aus der Vorgeschichte der psychologischen Optik.' [Outlines the views of the Greek, Alexandrian and mediaeval periods (Aristotle; Euclid; Alhazen, Vitello, R. Bacon).] E. R. Jaensch. 'Zur Methodik experimenteller Untersuchungen an optischen Anschauungsbildern: z. T. nach gemeinsam mit F. Reich durchgeführten Versuchen.' [The images of sense-memory follow the general laws of perception: identity of visual direction, retinal incongruity. The eidetic *Anlage* is found in two types of constitution, the tetanic and the Basedow; most cases are clinically quite normal.] D. Katz. 'Psychologische Versuche mit Amputierten.' [Summarises observations on the illusion of the phantom limb, on the sensitivity of the stump, and on the behavior of muscles after Sauerbruch's opera-

tion. The perception of weight arises, not in the joints, but in muscle and tendon.] O. KROH. 'Eidetiker unter deutschen Dichtern: ein Betirag zum Problem des dichterischen Schaffens.' [Discusses the images of sense-memory in the cases of O. Ludwig, L. Tieck, E. T. A. Hoffman, J. V. von Scheffel, and Goethe.] G. RÉVÉSZ. 'Prüfung der Musikalität.' [Describes and evaluates tests of rhythmical sense, absolute and relative ear, recognition of octaves, analysis of dyads, triads, etc., the catching and repeating of melodies, playing by ear.] E. RUBIN. 'Vorteile der Zweckbetrachtung für die Erkenntnis.' [A teleologically directed *Aufgabe* has the advantages of cumulative pleasure, analytic procedure, results from unsystematic work, availability of organised memory-material.] F. SCHUMANN. 'Die Repräsentation des leeren Raumes im Bewusstsein: eine neue Empfindung.' [Reduces vision of empty space (as in stereoscopic combinations) to a true sensation, the 'glassy' sensation.] W. BAADE. 'Zur Lehre von den psychischen Eigenschaften.' [Psychology has two parts, which deal respectively with the phenomena of consciousness and with the psychical attributes of the organism. These attributes are in turn either general, whose adaptive reactions bear witness to a principle of continuity, or circumscript, whose functions are approximately stereotyped. The writer advocates a special dynamology, or science of the circumscript attributes (Beneke's *Angelegtheiten*, Wundt's psychical dispositions, Müller's residues).]

Bd. lxxxv., Heft 5, 6. L. BOUMAN und A. A. GRUENBAUM. 'Kasuistischer Beitrag zur Vorstellungspsychologie.' [The self-centred daydreams of a patient who, under laboratory tests, shows small power of visualisation are made up of vivid visual, auditory, haptical images. There are thus objective and personal types of ideation.] J. PLASSMANN. 'Säkulare Veränderlichkeit des Dezimalfehlers.' [Continues records from 1917 to 1919.] H. KELLER. 'Eine Verbesserung am Hippschen Chronoscop.' [Rotary dial.] Literaturbericht.

Bd. lxxxvi., Heft 1-3. W. FUCHS. 'Untersuchungen über das Sehen der Hemianopiker und Hemiamblyopiker: ii. Die totalisierende Gestaltauffassung.' [Fifth paper of the Gelb-Goldstein series. Experiments show that, under certain conditions, central supplementation may give rise to the appearance of clear vision by the defective or injured parts of the retina. The conditions prove that the result cannot be explained by images or residua or attention; there is a 'totalising apprehension,' a touching-off of a central *Gesamtgestaltprozess*. This same process is normally at work in the filling-out of the blind spot, in twilight vision by the fovea, in certain observations of visual forms (Schumann, Zigler).] H. HENNING. 'Ein optisches Hintereinander und Ineinander: Gemischte Farbenempfindungen.' [(1) A simple stereoscopic arrangement permits the seeing of two colored areas at different distances with corresponding (identical) retinal areas; the nearer color is a transparent surface. (2) A modification of von Karpinska's apparatus permits the seeing of differently colored lines (under certain circumstances, areas), with corresponding retinal areas, at identically the same place (*am gleichen Orte der Kernfläche*); the result depends upon accurate fusion of contours and energy of attention. (3) A *Nachtrag* discusses related phenomena and physiological conditions.] *Literaturbericht*. Gesellschaft für experimentelle Psychologie. [Notice of 7th Congress.]

Bd. lxxxvi., Heft 4. E. KAILA. 'Eine neue Theorie des Aubert-Försterschen Phänomens.' [Haploscopic experiments show that the Aubert-Förster phenomenon (optical figures that are small and near are recognized over a larger portion of the field of vision than large and distant figures appearing under the same visual angle) is independent of accommodation and convergence; Jaensch's theory is thus refuted. In point of fact, distant objects arouse more residua than near, and this arousal means mutual inhibition, which is therefore the key to the phenomenon. The Koster phenomenon (enhanced *Eindringlichkeit* in micropsia, and under conditions which destroy visual objectivity) is to be explained similarly, as due to the cutting-off of residua.] K. WINZEN. 'Die Abhängigkeit der paarweisen Assoziation von der Stellung des besser haftenden Gliedes.' [Varied experiments prove that, if two ideas are to be associated, and the one is more holdfast (*besser haftend*) than the other,—more familiar, more insistent,—it is of advantage for retention that the more holdfast idea come first and not second. Hence in learning vocabularies one should place the foreign word to the right, not (as is usual) to the left of the native word.] W. HELLPACH. 'Zu der Bemerkung des Herrn Brugmans in Heft 5/6 des 85. Bandes dieser Zeitschrift.'

Bd. lxxxvi., Heft 5, 6. F. SCHUMANN. 'Die Dimensionen des Raumes.' [Second paper of the series Untersuchungen über die psychologischen Grundprobleme der Tiefenwahrnehmung, ed. by Schumann. —Raises the question whether the truly plastic spatial impression, as distinguished from that of a pictorial representation of space, is in fact tridimensional. Recent experiments have made it certain that two colors may appear at different distances in the same visual direction; and this fact, together with the existence of the colorless glassy sensation, seem to assure tridimensionality. The conclusion, however, is so far only tentative.] E. R. JAENSCH und F. REICH. 'Ueber die Lokalisation im Sehraum.' [Second paper in the series Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter, ed. by Jaensch.—(1) The localisation of the images of sense-memory (*Anschauungsbilder*), whether with steady fixation or with shift of the point of regard, is determined by the region of attention (*Aufmerksamkeitsort*). (2) All the phenomena observable with the use of the three actual threads of the Hering-Hillebrand fundamental experiment on horopter deviation are also observable in the parallel experiment with images of sense-memory; the same three types of observer appear, and in general an observer retains his type in passing from the one to the other experiment. If fixation is long maintained, the phenomena of the fundamental experiment are typically complicated by the supervention of images. These results suggest the use of the image as an instrument of analysis of the fundamental experiment; and it is found that if, in the observation of two or three imaginal threads in the image of sense-memory, one or two are distinguished by the attention, then, as the attention is voluntarily shifted, the preferred thread or threads shift also in the same direction. Since a near-position of attention touches off a tendency to adjustment of attention for distance, and conversely, the whole group of phenomena falls under Hering's principle of the self-regulation of living substance (cf. Mach's antagonistic processes of attention). (3) Hillebrand's explanation of the horopter deviation is unsatisfactory because his experimental arrangements were only partial. Space-values are not stable. The cue to explanation lies in the gross phenomena of the

parallel experiment; and the passage from eidetic (imaginal) through hemieidetic to rudimentarily eidetic endowment furnishes the needed continuity between the parallel and the fundamental experiments.] A. HOEFLER. 'Meinongs Psychologie.' [An appreciation of the ten papers in the psychological volume of Meinong's *Gesammelte Abhandlungen.*] Literaturbericht.

Bd. lxxxvii., Heft 1, 2. E. BECHER. 'W. Köhlers physikalische Theorie der physiologischen Vorgänge, die der Gestaltwahrnehmung zugrunde liegen.' [Popular paraphrase of the argument of Köhler's book "Die physischen Gestalten in Ruhe und im stationären Zustand" (1920), with comment interspersed in smaller type: a useful paper.] G. MARZYNSKI. 'Studien zur zentralen Transformation der Farben. [(1) A given grey in a given illumination may be objectively lightened or darkened *either* by addition of white or black *or* by increase or decrease of total or partial illumination; the resulting difference of intensity of light may then be the same, but phenomenologically the change of brightness is not identical with the change due to total or partial 'transformation.' An examination of the experimental studies of Weber's Law shows that constancy of the differential sensitivity appears only where transformation is in play. (2) A shadowed and an unshadowed paper may be compared for objective, subjective and reductive equality. The shadowed paper looks darker under the subjective than under the objective attitude, but still appears less dark than the reductive procedure makes it. Schools of painting show characteristic differences in their treatment of shadows.] E. GOTTHEIL. 'Ueber das latente Sinnengedächtnis der Jugendlichen und seine Aufdeckung.' [Second paper of the series Ueber die Vorstellungswelt der Jugendlichen und den Aufbau des intellektuellen Lebens, ed. by Jaensch.—Tests made by way of after-image and memory-image bring out rudiments of images of sense-memory even in cases where a direct test fails. An eidetic phase of development is therefore in all probability normal.] E. R. und W. JAENSCH. 'Ueber die Verbreitung der eidetischen Anlage im Jugendalter.' [Third paper of the series.—A statistical enquiry confirms the results of the preceding study.] A. GOESSER. 'Ueber die Gründe des verschiedenen Verhaltens der einzelnen Gedächtnisstufen.' [Fourth paper of the series.—Experiments on after-images, images of sense-memory, and memory-images, as regards mode of appearance of background, freedom from background, influence of a rotating or uneven background and of a colored projection-surface, and assimilation to objects of perception, all alike indicate that as the memory-series proceeds the connection of the image with the objects of perception simultaneously presented becomes weaker.]

Bd. lxxxvii., Heft 3, 4. B. HERWIG. 'Ueber den inneren Farbensinn der Jugendlichen und seine Beziehung zu den allgemeinen Fragen der Lichtsinns.' [This paper, the fifth of the series Ueber Grundfragen der Farbenpsychologie ed. by Jaensch, opens with a full discussion of the nature and distribution of the image of sense-memory, its differences from the ordinary after-image, the conditions of its appearance as positive or negative, etc. The author then shows that many sense-phenomena appear in the image in exaggerated and therefore more readily accessible form. The study of these imaginal effects is therefore instructive for the general psychology of vision: they may help us, *e.g.*, to distinguish color-weakness from color-blindness, inner from

outer color-blindness, etc. The phenomena of induction receive special attention, and the normal color-blindness of the peripheral retina is referred to induction rather than to anatomical insufficiency.] E. R. JAENSCH. 'Ueber Kontrast im optischen Anschauungsbild.' [Sixth paper of the series.—Contrast-effects are enhanced in the image.] B. HERWIG und E. R. JAENSCH. 'Ueber Mischung von objektiv dargebotenen Farben mit Farben des Anschauungsbildes.' [Seventh paper. —Phenomena of mixture, less often of rivalry, occur. The results of mixture confirm the status of the image.] Literaturbericht.

Arch. f. d. ges. Psychologie. Bd. xxxix., Heft 3, 4. B. PAULSSEN. 'Einfache Reaktionen bei Variation und rhythmischer Gliederung der Vorperiode.' [The effect of equally spaced auditory stimuli in the foreperiod of auditory reaction differs with the length of the intervals and the tendency of the reactor to rhythmisation. The best result (unity of voluntary process, correct reaction) is obtained with two optimally spaced signals which are apprehended with the stimulus proper as forming an anapaest.] J. O. VERTES. 'Das Gedächtnis der Blinden.' [The immediate verbal memory of blind children is better, both in range and in time of reproduction, than that of normal children.] J. K. von HOESSLIN. 'Die Melodie as gestaltender Ausdruck seelischen Lebens.' [Working from the definitions of melody offered by Lipps and Siebeck, the writer finds that tones affect the inner life of mind both by their manifold interrelations and by the play of tensions which they touch off. The tensions are enough: we have space-melodies in the Gothic cathedral and thought-melodies in the poems of Goethe and Lenau.] E. STERN. 'Zur Frage der "logischen" Wertung.' [Experiments on touch, taste and smell (right-wrong, present-absent) confirm Haering's thesis that cognition is an evaluation.] W. WIRTH. 'Beiträge zur psychophysischen Anthropologie. i. Anomalien der Gesichtsfarbe als Begleiterscheinungen der Farbenblindheit.' [Deuteranopes seem to be markedly ruddy, protanopes pale.]

Bd. xl., Heft 1, 2. C. THEODORIDIS. 'Sexuelles Fühlen und Werten.' [The author starts from the thesis that moral ideas and social organisation have their root in sexual evaluation, and that universally and originally the sexual act is regarded as something mysterious, forbidden, impure: this is the view of the man, who considers that he injures or defiles the woman. After adducing evidence for his views, the author considers certain problems of social psychology, connected with the beginnings of society and with the growth of law.] N. ACH. 'Zur Psychologie der Amputierten: ein Beitrag zur praktischen Psychologie.' [Discusses the mental state of the patient, and his psychological treatment; the choice and use of the prosthesis; and the education of the will to work.]

Bd. xl., Heft 3, 4. W. WIRTH. 'Unserem grossen Lehrer Wilhelm Wundt in unauslöschlicher Dankbarkeit zum Gedächtnis!' [Memorial article with two portraits.] O. KLEMM. 'Untersuchungen über die Lokalisation von Schallreizen: 4. Ueber den Einfluss des binauralen Zeitunterschiedes auf die Lokalisation.' [Discusses the binaural temporal limen, the appearance of a simple subjective field of hearing, localisation and apparent movement in the sense of the temporal difference. Experiments with the Helmholtz pendulum give extraordinarily small times: thus localisation is possible for one observer with a temporal difference of 0.002σ.] E. SCHERER. 'Das Problem der anschaulichen Gestaltung in der Lyrik.' [The sensory and affective

reality that is aesthetically characteristic of the best lyric poetry depends, not on the arousal of visual imagery, but upon verbal melody and rhythm: pitch, weight, volume of vowels, duration, accent of syllables. These elements, possibly reinforced by kinaesthesis, are responsible for the plastic effect.] J. E. LIPS. 'Die gleichzeitige Vergleichung Zweier Strecken mit einer dritten nach dem Augenmass: zum Drei-Reize-Problem in der Psychophysik.' [Under the experimental conditions the double judgment has a small advantage in accuracy over the corresponding single judgment. Under the same conditions the normal *DL* is approximately the same as compared with the current values of the simultaneous *DL;* new experiments, however, greatly reduce these latter.]

Psychological Review. Vol. xxvii., no. 1. C. E. FERREE and G. RAND. 'The Absolute Limits of Colour Sensitivity and the Effect of Intensity of Light upon the Apparent Limits.' [With stimuli of high intensity, the limits of R, B, Y coincide with those of white-light vision (the limits for G are narrower); for stimuli of equal energy at medium intensities, the limits of R, B, Y interlace. In the mid and far periphery of the retina, large differences in amount of light are needed to change perceptibly the limits of sensitivity.] H. A. CARR and M. C. HARDY. 'Some Factors in the Perception of Relative Motion: A Preliminary Experiment.' [Observations on two lights, set vertically, either one of which could be given a horizontal movement. Perceptive accuracy is increased by difference in size (or increase in combined area, or both); by inequality of brightness and decrease of combined illumination; by rate of motion; and (in general) by fixation of the stationary light. Extent of motion has no influence, and position of stationary light a variable effect.] S. D. ROBBINS. 'A New Objective Test for Verbal Imagery Types.' [Threefold presentation of specially prepared meaningless syllables. Consonants receive less attention than vowels, and short vowels much less than long. Vivid verbal imagery makes for efficiency.] 'Note on the Verbal Imagery of Stammerers and Normal Speakers.' [Stammerers pay more attention than normal speakers to consonants.] J. R. KANTOR. 'A Functional Interpretation of Human Instincts.' [We must distinguish between instinct (functioning of a connate potential reaction system) and instinctive behaviour (which comprises acquired tendencies of response). We then understand that all instincts are specific; that not a single act of an adult person is instinctive (an original response); and that the emotive situation, by dissociating reaction systems, may leave in function only some instinctive mode of behaviour.] J. P. M'GONIGAL. 'Immobility: An Enquiry into the Mechanism of the Fear Reaction.' [The expression of fear is primarily an effect of thyrine, as that of anger is an effect of adrenin.]

Psychological Review. Vol. xxvii., No. 2. W. D. SCOTT. 'Changes in Some of our Conceptions and Practices of Personnel.' [Emphasises individual differences, the non-rational aspects of behavior, the definition of education as profiting by experience, the idea of workers-in-their-work as opposed to that of pegs and holes, the scientific basis of vocational guidance.] J. J. B. MORGAN. 'An Analysis of Effort.' [Effort is fundamentally a reflex response to an inimical stimulus which the normal organic response has failed to meet.] J. F. DASHIELL. 'A Comparison of Complete *vs.* Alternate methods of Learning Two Habits.' [Experiments under varied conditions on maze

running (rats, children, adults), card-sorting and adding (adults), show that learning by the complete method is the more economical procedure.] R. M. OGDEN. 'The Tonal Manifold.' [Graphic representation of the system of pitch-brightness, volume and intensity.] C. ROSENOW. 'Is Lack of Intelligence the Chief Cause of Delinquency?' [The correct conclusion from Goring's statistics is that in all probability factors other than intelligence are of greater importance as determinants of crime.]

Vol. xxvii., No. 3. C. SPEARMAN. 'Manifold Sub-Theories of "The Two Factors."' [Expands reply to Thomson in *Brit. J. Psych.*, 1916. The fundamental theory has now been demonstrated with finality.] G. H. THOMSON. 'General *vs.* Group Factors in Mental Activities.' [The proof of the Theory of Two Factors by hierarchical order falls to the ground, and Spearman's mathematical argument is finally proved invalid. A Sampling Theory of Ability, which considers any performance as carried out by a sample of group factors, is offered as alternative.] J. R. KANTOR. 'Suggestions toward a Scientific Interpretation of Perception.' [Perception is the conscious behavior through which are developed the meanings of objects and relations which operate in the adaptation of the individual to his surroundings and in their control.] E. C. TOLMAN. 'Instinct and Purpose.' [Instincts are defined as (1) determining adjustments, often hierarchically arranged, which set in readiness particular groups of (2) variable subordinate acts. Purpose is the interaction of (1) and (2).] S. B. RUSSELL. 'Brain Mechanisms and Mental Images' [Mental images occur only when there is coordinated molecular response in a brain centre conditioned by a former environment. The response is conditioned by means of registering mechanisms in the centre and association mechanisms.]

Vol. xxvii., No. 4. W. S. HUNTER. 'The Modification of Instinct from the Standpoint of Social Psychology.' [The social significance of instinct depends largely on the modification (structural, temporal, adaptive) that instinctive forms of behavior undergo under the influence of intelligent behavior.] E. ISAACS. 'The Nature of the Rhythm Experience.' [Rhythm is the experience arising from the periodic, pendular, reflex response of characteristic organs to objective stimulation. It thus involves perception of stimuli, experience of periodic reflex response, accentuation and grouping by attention, and feeling due to repeated movement.] G. H. THOMSON. 'A New Point of View in the Interpretation of Threshold Measurements in Psychophysics.' [Emphasises the influence of 'moral' characteristics or 'mood' upon the *DL*.] J. W. BRIDGES and V. M. DOLLINGER. 'The Correlation between Interests and Abilities in College Courses.' [On the face of the returns relative interests are an extraordinarily inaccurate symptom of relative capacities; but the problem is highly complex.] R. H. WHEELER. 'Visual Phenomena in the Dreams of a Blind Subject.' [After sixteen years of blindness the dreams show three peculiarities; the persistence of the synaesthesia of the waking life, the reduction of voices to the subject's own vocal-motor imagery and their differentiation by color, and a tendency to visualise himself at a distance.]

Vol. xxvii. No. 5. L. T. TROLAND. 'The Physical Basis of Nerve Functions.' [Discusses, on the basis of the work of Nernst, Lillie and Lucas, the general mechanisms of excitation and stimulation, the specific mechanisms of the threshold, impulse propagation, the ener-

getics of nerve processes, the all-or-none principle, and the mechanisms of synapse and receptor; and makes psychophysiological applications.] R. H. WHEELER. 'Theories of the Will and Kinaesthetic Sensations.' [The historical divergence of opinions is due to the prejudice in favor of an unique mental process, to systematic bias and to lack of introspective data. The 'unique mental process' in will is simply kinaesthesis.] W. R. MILES. 'A Pursuit Pendulum.' [The instrument enables us to measure quickness, precision, steadiness of movement in reference to a moving object.] C. E. FERREE and G. RAND. 'The Limits of Color Sensitivity: Effect of Brightness of Preexposure and Surrounding Field.' [Color zones are widest when preexposure and surrounding fields have the brightness of the color. Difference of surrounding field may narrow a zone by 11°; of preexposure field, by 17°; of both together, by 20°.]

Vol. xxvii., No. 6. A. S. OTIS. 'Do We Think in Words?' [Thought is restricted neither to the material of language nor to the action of the language mechanisms.] C. H. WOOLBERT. 'A Behavioristic Account of Sleep.' [Sleep is a matter of the efficient domination of the upper muscular systems by the lower, operating through the relaxing power of fatigue.] E. S. ROBINSON. 'The Compensatory Function of Make-Believe Play.' [Play, the more private forms of fantasy, much lying and story-telling, and the appreciation of stories all serve the same fundamental purpose as compensatory mechanisms.] E. G. BORING. 'The Control of Attitude in Psychophysical Experiments.' [Gives rules, in reply to Thomson, for the regulation of attitude in psychophysical experiments.] L. A. JONES and P. REEVES. 'The Physical Measurement and Specification of Color.' [Discusses uses of spectroradiometer, spectrophotometer, colorimeter.] S. L. PRESSEY. 'Suggestions Looking toward a Fundamental Revision of Current Statistical Procedure as Applied to Tests.' ['Reliability' simply means consistency, and 'validity' does not assure value. Tests should be built specifically for concrete problems, and their value measured by their efficiency in dealing with these problems.]

Vol. xxviii., No. 1. M. W. CALKINS. 'The Truly Psychological Behaviorism.' [Neither Watson nor Warren, but only the self-psychologist, may lay claim to a genuine behavioristic psychology, a study of the totally integrated individual in the attitudes whereby it confronts its environment.] J. R. KANTOR. 'An Attempt toward a Naturalistic Description of Emotions, i.' [The distinctive mark of emotion is the absence in the act of an organised response-system; emotion is a momentary condition of 'no-response.' Analysis reveals three phases: an act of simple apprehension, disintegration of the constitutive response-systems (emotive activity proper), and superseding organic or other activities. Since emotion appears only under definite external conditions the question of inheritance is futile.] H. LUNDHOLM. 'The Affective Tone of Lines: Experimental Researches.' [Lines appear to imitate in their movement the motor expression of emotions, and therefore arouse feeling-tone.] S. C. PEPPER. 'The Law of Habituation.' [Secular changes of appreciation, 'value mutations,' are accounted for by a law of habituation. Under repeated stimulation, the members of a linked affective series swing, continuously or cyclically, from dislike to liking.] W. SCHILLING. 'The Effect of Caffein and Acetanilid on Simple Reaction Time.' [The drugs, in 5-grain doses, retard reaction time and increase unsteadiness. Nothing positive can be said of an effect on pulse and breathing.]

NOTES

ON THE PLAN OF THE *PHYSIOLOGISCHE PSYCHOLOGIE*

The letter of Decr. 8th, 1872, in which Wundt offered the *Physiologische Psychologie* to the firm of Engelmann, contains an outline of the work, which shapes as follows:

I. Physiological Properties of the Nervous System
II. Sensation and Idea
III. Organic Movements
IV. Critique of Psychological Doctrines
V. General Theory of Psychophysical Occurrence

Since II and III comprise "the empirical material of physiological psychology proper"—"the inner or psychological and the outer or physiological consequence of the manifold interactions between our outer and inner experience"—the schema may be rewritten thus:

I. Physiology
II. Physiological Psychology
III. Psychology
IV. Theory of the Relation of Inner to Outer Experience

where the *Physiology* stands for relevant, *i.e.*, neural physiology; the *Psychology* stands mainly for the trends (Wolff and Kant, Herbart and Beneke) against which Wundt was struggling; and the concluding *Theory* discusses, in a wide sweep, "the relation obtaining in the last resort between the worlds of inner and of outer occurrence, and the explanation of the whole interconnection of psychological phenomena suggested by the survey of their borderland." The programme is clear and logical.

The published book is built, however, on a different plan. I shall first give its contents *with one omission.*

1873. Introduction, pp. 1-20.
I. Physiological Properties of the Nervous System, pp. 21-272.
[A. Inner Aspect of Physiological Psychology]
II. Sensation and Sense-Feeling, pp. 273-463.
1874. III. Idea and Aesthetic Feeling, pp. 464-706.
IV. Association of Ideas and Emotion, pp. 726-819.
[B. Outer Aspect of Physiological Psychology]
V. Movements, pp. 820-858.
Conclusion, pp. 858-863.

It is clear that what I have called the inner aspect of physiological psychology is represented, not at the one combined level of the original schema, but at three successive levels, at each one of which the 'objective' experience has its 'subjective' pair. It is clear, also, that the increased space required for this elaboration has meant the curtailment of Parts III, IV and V of the original outline: movement gets only a scant 40 pages, as against over 600 for its co-ordinate Part; the psychological criticisms are packed away in brief appendices to the successive chapters; and the wide-sweeping Conclusion is reduced to exactly five pages! No doubt, the publisher had his say; Wundt had estimated the size of the work at 640 to 800 pp., and it runs to 872. At all events, the mould was now set. Movement never recovers its lost importance; psychological criticism remains

to the end a matter of appended paragraphs; and the full Conclusion appears only in the fifth and sixth editions.

So far, then, we have the familiar picture of a work which, through wealth of material, has grown under its author's hands until it outruns the appointed limit; the earlier parts are disproportionately long, the later disproportionately curtailed. But I have oversimplified; I have omitted pp. 707-725, the initial section of Part IV. These twenty pages, which are entitled *Bewusstsein und Aufmerksamkeit*, are of extraordinary significance. Their intervention, between the doctrine of idea and the doctrine of association of ideas, means that a second systematic thread, in addition to the thread of sensory integration, appears in all the following exposition.[1] We should expect a reference to it in the first, physiological Part; but there is no apperception-centre in 1873. We should expect, going further back, a reference in the Introduction; but there is no hint there of consciousness and attention. The fact is that Wundt, when he began to write the book, had no notion that he should presently introduce them. It was only when he came to work up the material of his nineteenth chapter, on the course and association of ideas,—his own experiments on reaction and complication, the results of Donders and Vierordt and the rest,—that, as the Preface informs us, he saw the possibility of further systematisation, of a theory of consciousness and attention which should, at any rate provisionally, round off an important division of physiological psychology. The opportunity thus presented was of precisely the sort that Wundt's genius welcomed. He set to work on the new (eighteenth) chapter; he found plenty of cues to back-reference, where there had been none to reference forward; and the doctrine of apperception was incorporated in the *Physiologische Psychologie*. It came in, nevertheless, by way of after-thought; and though it grew to overshadowing predominance,—the stages of that growth are another story,—we may doubt whether it was ever very firmly rooted; whether (to change the figure) it was ever really at home in the general systematic setting of the book.

E. B. T.

EXPERIMENTAL PSYCHOLOGY IN ITALY

In his inaugural lecture at the University of Palermo Professor F. U. Saffiotti sketches the development of experimental psychology in Italy.[2] The pioneers were three Sicilians, G. Sergi, G. Buccola and S. Corleo. Sergi as early as 1876 advocated the establishment of a laboratory; but nothing came of his efforts before 1889, when a laboratory was founded at Rome as a section of the Institute of Anthropology. Buccola, who died young, worked from 1880 to 1895 with A. Tamburini in the hospital of Reggio Emilia and with E. Morselli in the hospital and the psychiatrical clinic at Turin. Corleo, who died in 1891, started in 1889 a small laboratory, afterwards allowed to lapse, at the University of Palermo. Psychological work, during these early years, was done in hospitals (at Reggio Emilia, for instance, Tamburini and G. C. Ferrari founded a laboratory in 1896) and in physiological institutes (A. Mosso at Turin, M. L. Patrizi

[1] Cf. this JOURNAL, xxxii, 1921, 116 f.

[2] La evoluzione della Psicologia Sperimentale in Italia, *Rivista di Psicologia*, xvi, 1920, 129 ff. Cf. this JOURNAL, xv, 1904, 515 ff; xvi, 1905, 225 ff.

at Modena); lecture-courses at universities appear to have depended altogether on the initiative of the professor (Sergi at Messina in 1878-9, Ferrari at Bologna). The first independent university laboratory was opened in Florence, 1903-4, by F. De Sarlo.

In 1905 the fifth international congress was held at Rome. A direct result was the establishment of three chairs of experimental psychology: S. De Sanctis was put in charge of the laboratory at Rome, F. Kiesow of that at Turin, and C. Colucci received the call to Naples. In 1908 a laboratory of scientific pedagogy at Crevalcore was transformed into a laboratory of pure and applied psychology and, as a communal institute of Milan, placed under the direction of Z. Treves. At the director's death in 1911 this laboratory seems to have declined; his personally owned apparatus were bequeathed to De Sanctis' laboratory at Rome,—where, curiously enough, the psychological section of the Anthropological Institute was still maintained under Sergi's guidance. In 1912 a psychological laboratory, named in honor of Treves, was started by Gonzales and Corberi in connection with the provincial hospital of Milan at Mombello. Finally, Saffiotti was called in 1918 to Palermo, and V. Benussi, apparently in the same year, to Padua.

The *Revista di Psicologia* was started by Ferrari in 1905; it is the organ of the *Società Italiana di Psicologia*, which took shape in 1911. In 1920 appeared the first numbers of the *Archivio Italiano di Psicologia*, edited by F. Kiesow of Turin and A. Gemelli, a pupil of Külpe and Kiesow, now in charge of the laboratory of the Instituto Nazionale Medico-Pedagogico at Milan. Volumes of experimental studies have been issued from Reggio Emilia, Rome, Florence and Turin. E. B. T.

THE PSYCHOPHYSIOLOGY OF THE CONDEMNED

Dr. L. Gualino, director of an Italian war-hospital, has published a paper on the psychophysiological characters of soldiers condemned to be shot for breach of discipline.[1] Pulse is accelerated to 100 at the moment of sentence; is thereafter variable; and sinks to 60 at the place of execution. Sweating is profuse, but a thermal anaesthesia prevents reaction to cold. Salivary secretion is lessened; the voice roughens or fails; tears cannot be shed. Breathing is of the Sikorsky type: the thorax is violently filled or emptied, and the succeeding respiratory movements are superficial and hardly if at all perceptible. There is no impulse to defecate, and no tendency to frequent and scanty urination; a vesical anaesthesia leads, however, to unnoticed overflow of the bladder's contents. The most characteristic physical symptom is a paresis or paralysis of the lower limbs. The face assumes a set, mask-like expression. The pupil alternates between dilatation and normality; in both conditions it reacts normally to light. There is trembling, but (the writer thinks) no true tremor; muscular contractions appear irregularly at various parts of the body, and are probably due to "multiple fibrillary myoclonias." Reflexes are never normal; they may be hightened or diminished. The various modes of general sensitivity show a loss of acuity which may reach actual anaesthesia. The specific sensitivities are rather heightened than impaired.

[1] Psicofisiologia dei fucilandi: Annotazione obbiettive, *Rivista di Psicologia*, xvi, 1920, 42 ff. The editor, Professor G. C. Ferrari, adds a brief note (101 ff.) entitled Psicologia dei moribondi.

The mental functions, on the other hand, maintain their integrity to the very end. Attention is lively, prompt and well-sustained. Memory is fully efficient. Thought, conception, judgment, reasoning present no change, qualitative or quantitative. The insistent idea is not that of death, or of what is to follow death, or of injustice, but of the unimaginable pain that may attend the moment of dying; the condemned are eager for fact or argument that may reassure them. Nor are the emotions blunted. The principal egoistic emotion displayed, apart from the overshadowing fear, is vanity; the condemned wish to make a good appearance, and are annoyed at the invading paresis; the chief altruistic emotion is a strong and variously directed sympathy. There is little sign of religious emotion. The religion of the condemned, like that of soldiers in general, is strictly utilitarian; their religious images are made, as circumstances suggest, the object of prayer or cursing; they seldom avail themselves voluntarily of the ministrations of the chaplain; and though at last they usually agree to receive the sacraments, this is due more to outside pressure and to the vague notion of a safeguard of the future than to any real religious feeling.

In his conclusion the author points out how far removed from the truth is the common belief that the condemned man is, to all intents, dead before he mounts the scaffold or takes his place on the field of execution. In natural death, it seems that the brain first succumbs, and that spinal paralysis follows later; in these cases of violent death, on the contrary, the cord appears to renounce its functions while the brain remains intact. E. B. T.

LOCOMOTION OF INSECTS

The locomotion of insects when walking with their six legs intact is roughly by three legs at a time, one set of three supporting the body tripod-wise while the other three are advancing. For example, while the right fore leg, the left middle leg and the right hind leg form the tripod of support, the left fore leg, the right middle leg and the left hind leg are advanced; then the last three form the support while the first three are advanced, and so on in alternation. This fact has long been known. Von Buddenbrock set himself to discover what happens when some of the legs (say the middle leg on each side) are removed by accident or amputation, using a walking-stick insect for his experiments.[1] If the original leg-partnerships were to continue, the insect would have a gait like a pacer, moving the fore and hind legs on the same side at the same time. As a matter of fact this is not what happens; but instead it uses the left fore leg with the right hind leg and the right fore leg with the left hind leg after the manner of a trotter, and does so whenever a leg on each side is removed, irrespective of their relation to each other. Removal of a single leg makes no change in the plan of locomotion.

The change of leg-usage when two legs are lacking is advantageous for an insect that must move about back-downward on the underside of leaves and the like, and raises the interesting question how the change is brought about. Is it due to the operation of some general center of locomotor control; is it caused in a purely mechanical way by the changed distribution of the weight of the insect upon the legs

[1] v. Buddenbrock: Der Rhythmus der Schreitbewegungen der Stabheuschrecke, Dyxippus; mit 2 Abbildungen. Biologisches Zentralblatt, Bd. 41, Nr. 1, Januar 1921, pp. 41-48.

that remain; is it a matter of a local neural mechanism? Von Buddenbrock concludes (on the basis of an extended series of experiments which involved, beside the amputations and ingenious tests of other leg-conditions, the sectioning or hemisectioning of the main ganglionic chain at various levels) that the last suggestion is the most probable. The change appears to be in reality no change of locomotor plan at all, but merely the operation of the original neural mechanism within which the relative excitabilities have been altered by the amputations.

For details the reader will wish to consult the original. It may be interesting, however, to note the following additional points with reference to the probable distribution of function within the nervous system of this insect. The *regulation* of the movements of locomotion seems to depend upon the three pairs of thoracic ganglia, but its *inception* upon the suboesophageal ganglion; the beheaded insect remains standing stock still. The head-ganglion or "brain," the receiving center for excitations from the organs of special sense, operates *inhibitively* for the most part, causing normally a quasi-cataleptic rigidity characteristic of the adult insect during the daytime; while removal of the "brain" results in almost continuous locomotor activity. E. C. S.

GEORGE TRUMBULL LADD

Professor Ladd was born at Painesville, Ohio, January 19, 1842, and died at New Haven on August 8 of the present year. He graduated from Western Reserve College in 1864, and from Andover Theological Seminary in 1869. From 1869 to 1879 he filled pastorates at Edinburg, Ohio, and Milwaukee, Wis.; in 1879 he was appointed professor of philosophy at Bowdoin College, and in 1881 he was called to Yale, where he remained in active service till 1906. He was a lecturer at Andover Theological Seminary in 1879-81; conducted a graduate seminary at Harvard in 1895-96; lectured in Japan in 1892 and 1899, and in India in 1899-1900; and was president of the American Psychological Association in 1893. His books, on theology, philosophy, and psychology, make a long list: his chief psychological works are *Elements of Physiological Psychology*, 1887 (second edition, in collaboration with R. S. Woodworth, 1911); *Philosophy of Mind*, 1891; *Primer of Psychology*, 1894; *Psychology, Descriptive and Explanatory*, 1894.

Ladd was not, in my judgment, a great psychologist; but he was competent and assiduous; and his publications were of very distinct service to the cause of psychology in this country. His *Physiological Psychology*, in particular,—I well remember my excitement on finding this book in the library of the Oxford Union, and the shock of disappointment at reading that mind was a real unit-being!—helped toward the establishment of laboratories and the recognition of experimental psychology as an academic study. Coming, as it did, from a professor of philosophy at Yale who had been a Congregational minister, it gave the young science an air of respectability (I can think of no better word) which was of high advantage in its struggle for life. Aside from this special value, however, the book was important as our one English text-book and book of reference. Those were primitive times: James' *Principles* were still three years away; the translation of Ziehen's little *Physiological Psychology* appeared in a first edition in 1892; and the first instalment of Sanford's *Course*

came out in 1894. Even in Germany Wundt's *Physiologische Psychologie* was only just attaining its third edition, and Münsterberg's *Willenshandlung* had not yet seen the light. Ladd had no model save the Wundt of 1880, and his volume embodies an amount of hard work for which we owe him grateful thanks.

By a coincidence which I wish might have been avoided this number of the JOURNAL contains a detailed criticism of Ladd's systematic psychology. I had looked forward to a trenchant rejoinder.

E. B. T.

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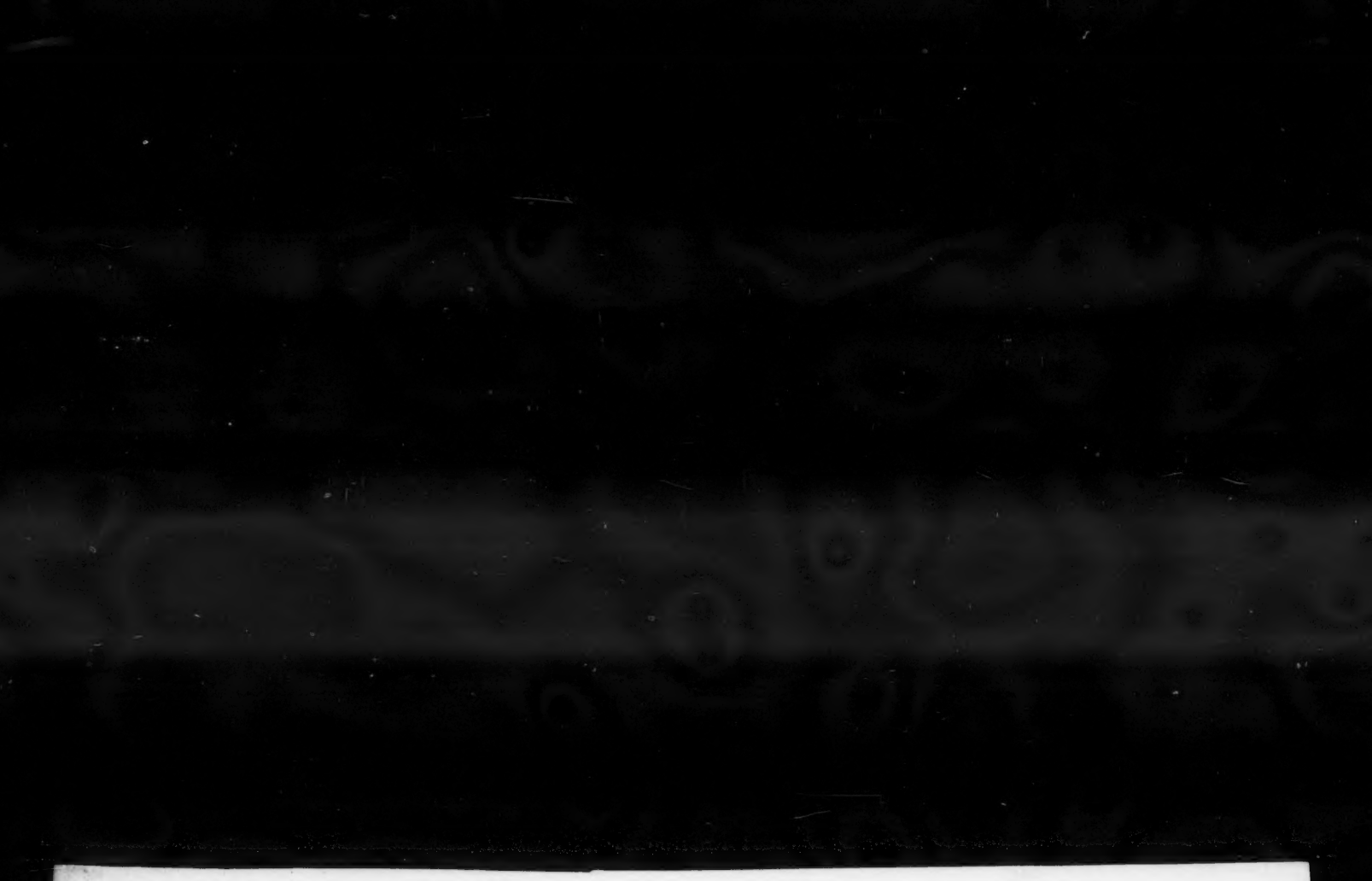
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